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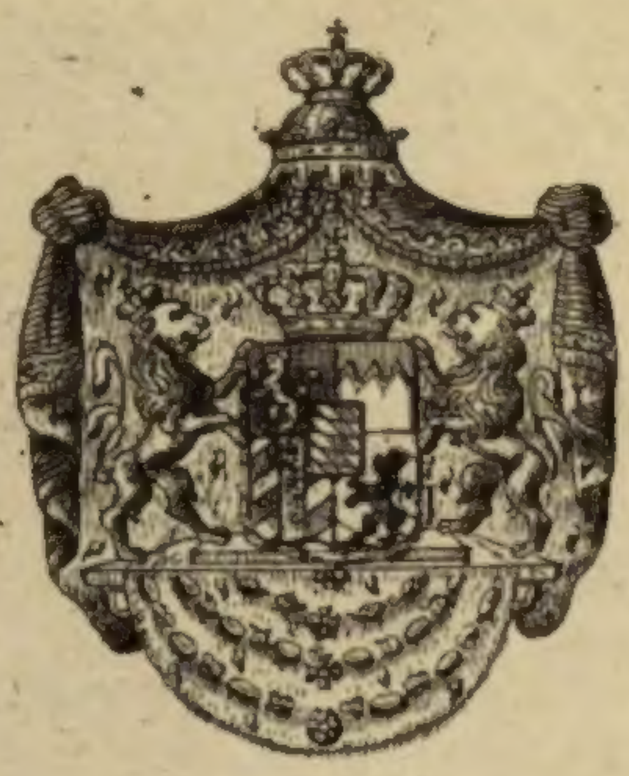

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AN

ESSAY ON THE BLOOD,

COMPREHENDING

THE CHIEF CIRCUMSTANCES WHICH INFLUENCE ITS COAGULATION;

THE NATURE OF THE BUFFY COAT;

WITH A CONCISE MEDICAL VIEW OF THE

STATE OF THE BLOOD IN DISEASE;

AND

AN ACCOUNT OF THE POWERS OF A SATURATED SOLUTION OF ALUM,

AS

A STYPTIC REMEDY IN HEMORRHAGE.

BY

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LONDON:

PRINTED FOR THE AUTHOR,

By Joseph Mallett, 59, Wardour Street, Soho;

AND PUBLISHED BY

LONGMAN, HURST, REES, ORME, BROWN, AND GREEN,

PATERNOSTER ROW.

1824.

Bayerische Staatsbibliothek München

ESSAY ON THE BLOOD,

Bayerische
Staatsbibliothek
München

PREFACE.

ALTHOUGH I have endeavoured, in the composition of this Essay, to include every important fact relating to the coagulation of the blood; I have purposely confined myself to an outline of the subject, in most of my investigation of a chemical nature. In regard to my general account of the experiments, I may observe, that I have related only a sufficient number to serve the purpose of showing the facts and material results; but, upon almost every point of investigation, they have been much more numerous than I have stated.

It must be allowed that a great part of the inquiry in which I have engaged, has necessarily

been attended with peculiar difficulty and trouble. It may be called a path of labour more useful than agreeable; except as the search after truth always carries with it some reward.

The pathological part of my subject will allow of being much extended; but demands, for this purpose, further study and observation. An acquaintance with the appearances and actual condition of the blood in disease, must surely be a very important guide to medical practice. In the treatment of active inflammation, we are directed in the most essential points, by the symptoms and particular state of the patient; but, even here, we may learn much from the characters of the blood; and in the management of chronic inflammation, and some obscure forms of disease, in which the repetition of venesection becomes doubtful, we derive, from a knowledge of the nature of the blood, a very material assistance to our judgment.

I trust that the grounds will appear sufficiently convincing, on which I have recommended the saturated solution of alum as a styptic applica-

tion to bleeding vessels. The internal use of this salt, in free doses, also appears to me deserving of confidence; and it possesses the merit of being perfectly innocent; a praise which cannot be given to the preparations of *lead*, the remedy commonly chosen in urgent cases of hemorrhage.

I beg leave to observe, in conclusion, that if the present Essay meet with a favourable reception, I shall be desirous, not only to prosecute my study of the subject, but to attempt, at some future period, a more extended dissertation.

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AN

ESSAY ON THE BLOOD,

&c. &c.

The difference in the external appearance of blood, as derived from the arteries or from the veins, is equally important and curious. It is chiefly important, as discovering at first sight to the physician and the surgeon the distinct nature of a hemorrhage, and suggesting consequently the relative treatment to be adopted. It is curious, as involving the inquiry, how the blood, which is returned to the heart by the veins, of a dark and almost black colour, ac-

quires, in its passage through the lungs, that beautiful scarlet hue which distinguishes it as arterial blood, fitted to promote the functions of the secreting and other organs of the body, and to supply nutrition to its remotest parts.

The separation of the blood, when drawn from the vessels of the living body, into a solid mass called crassamentum or cruor, and the fluid portion called serum, is a fact too familiarly known to be dwelt upon in this place.

As preliminary to my more immediate subject, I shall take the liberty of quoting, from the latest and best authorities, some account of the chemical constitution of the blood.

The following is borrowed from Mr. Brande's Manual of Chemistry*.

" The specific gravity of the serum of the blood, is upon an average 1·030. It reddens the yellow of turmeric, and changes the blue of violets to green, a property derived from a portion of soda. At a temperature of 160°, it be-

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* See article Blood, vol. 3.

comes a firm yellowish white coagulum, resembling in appearance and properties the coagulated white of egg, and, as the principle to which this property is owing is the same in both substances, it has been called *albumen*. Alcohol, and many of the acids, also occasion the coagulation of the serum of blood.

" 100 parts of human serum contain between eight and nine parts of albumen, rather less than one part of carbonate of soda, and about the same quantity of common salt, the remaining 90 parts being water. These at least are proportions which my own experiments lead me to believe correct; but the analysis is involved in so much difficulty, that the results can only be considered as approximating to the truth; indeed it is probable that the composition of the serum is liable to much variation.

" Dr. Marcet and Berzelius have each given an analysis of the serum of human blood; the following are their results. (*Medico-Chirurgical Transactions*, vol. ii. *Annals of Philosophy*, vol. ii.):

MARCET.

Water	900·
Albumen	86·8
Muriates of potassa and soda	6·6
Muco-extractive matter	4·0
Carbonate of soda	1·65
Sulphate of potassa	0·35
Earthy phosphates	0·60
	1000·00

BERZELIUS.

Water	905·0
Albumen	80·0
Muriates of potassa and soda	6·0
Lactate of soda, with animal matter	4·0
Soda and phosphate of soda with ditto	4·1
Loss	0·9
	1000·0

" *Albumen*, which constitutes a leading ingredient in the serum, and which we shall presently find also in the cruor, is a very important

animal principle, and is found in the greater number of animal fluids and solids.

" *Liquid Albumen* is soluble in water, and always contains a notable portion of soda, indicated by its action on vegetable colours. It is coagulated by heat, acids, and alcohol, unless it be considerably diluted with water, in which case a portion separates in the form of white flakes after some hours' standing. Solution of corrosive sublimate, added to albumen very much diluted, produces a cloudiness, and hence it is a useful test of albumen. (Bostock, Nicholson's *Journal*, xiv.) It is also instantly coagulated by Voltaic electricity; and if two platinum wires connected with a small battery be immersed into a diluted albumen, it will cause a very rapid coagulation at the negative pole, and scarcely any effect at the positive pole. This circumstance induced me to attribute the coagulation to the removal of the alcali, by alcohol, and by acids; but how heat operates is not very obvious, unless we be allowed to consider it as effecting a kind of decomposition of the liquid albumen.

We might thus consider liquid albumen as a compound of albumen and soda dissolved in water: the effect of heat would then be to transfer the soda to the water, and thus occasion a coagulation; and a solution of soda is always found oozing from coagulated serum, and has sometimes been called *serosity;* in time it re-acts upon the coagulum, and dissolves a portion of it.

" When albumen is dried in a moderate heat, it shrinks and becomes brown and semi-transparent, resembling horn in appearance and properties. In this state it scarcely dissolves in boiling water, though it gradually softens; it is not prone to decomposition; it dissolves in the alcalis, a portion of ammonia being evolved and a saponaceous compound formed. Dilute nitric acid converts it into a substance having the properties of gelatine. (HATCHETT, *Philosophical Transactions*, 1800.)

" By destructive distillation albumen furnishes a variety of products characterized by the presence of a large proportion of ammonia. According to Gay-Lussac and Thenard (*Recherches*

Physico-Chymiques), its ultimate constituents are

Carbon..	52·883
Oxygen.....	23·872
Hydrogen.....	7·540
Nitrogen.....	15·705
	100·000

" When the coagulum of the blood is carefully washed under a small stream of water, the colouring matter is gradually dissolved, and washed out of it, and a white fibrous substance remains, which has been termed *fibrina*, or *coagulable lymph*, but of which the chemical properties are those of albumen."

" The colour of the blood has generally been referred to small globules of a red colour, which by the aid of the microscope may be discerned in it ; and it was supposed that these globules are soluble in water. But it has been shown by Dr. Young, that this is not the case, and that the effect of water is to dissolve the colouring matter only, leaving the globule perfectly colourless ; in

this state the globular particles have the properties of albumen. The diameter of the globules in human blood varies from $\frac{1}{8000}$ to $\frac{1}{4000}$ of an inch. — *Remarks on Blood and Pus, in* Dr. Young's *Medical Literature.*

" The colouring matter of the blood can scarcely be obtained free from other substances. By stirring it during coagulation, a considerable portion is diffused through the serum from which it afterwards subsides. Vauquelin advises the digestion of the coagulum, drained of serum, in dilute sulphuric acid, at a temperature of 160°. The liquid, filtered while hot, is to be evaporated to half its bulk, and nearly saturated with ammonia ; the colouring matter falls, and is to be washed and dried. (*Annales de Chimie et Physique*, tom. i.) We must not, however, trust animal principles to these complex operations ; and there can, I think, be little doubt that the colouring principle has undergone some change in M. Vauquelin's process.

" The chemical properties of the colouring

matter of the blood show that it is a peculiar animal principle."

In the Philosophical Transactions for 1812, Mr. Brande relates the following experiments, on which he founds his conclusions that the colour of blood is not owing to iron:

"Assuming the existence of iron in the colouring matter of the blood, I made the following experiments upon the crassamentum of that fluid.

"Two pints of blood were collected in separate vessels. The one portion was allowed to coagulate spontaneously, the other was stirred for half an hour with a piece of wood, so as to collect the coagulum, but to diffuse the principal part of the colouring matter through the serum. These two portions of coagulum were now dried in a water-bath, and equal weights of each reduced in a platina crucible to the state of coal, which afterwards was incinerated. The ashes were digested in dilute nitro-muriatic acid, and the solution saturated with liquid ammonia, in

order to precipitate the phosphate of lime as well as any iron which might have been present.

" The precipitates were collected, dried, and treated with dilute acetic acid, by which they were almost entirely dissolved, some very minute traces only of red oxide of iron remaining, the quantity of which was similar in both cases, and so small as nearly to have escaped observation.

" It is reasonable to infer, that, if the colouring matter of the blood were constituted by iron in any state of combination, a larger relative proportion of that metal would have been discoverable in the former than in the latter coagulum; but frequent repetitions of these experiments have shewn that this is not the case, and the following result appears to complete the evidence on this subject.

" The colouring matter of a pint of blood was diffused by agitation through the serum, from which it was allowed gradually to subside, the coagulum having been removed: after twenty-four hours, the clear serum was decanted off, and the remainder, containing the colouring matter,

after having been evaporated to dryness, was incinerated, and the ash examined as in former experiments. But the traces of iron were here as indistinct as in the other instances above mentioned, although a considerable quantity of the colouring matter had been employed."

Mr. Brande states, " that when the colouring matter is collected and microscopically examined, it seems, as Lewenhoeck first observed*, to consist of minute globules. These are usually described as soluble in water, a circumstance which my own observations led me to doubt, and which the more accurate experiments of Dr. Young, an account of which, intended for publication, he has kindly permitted me to peruse, have completely disproved.

" The effect of water upon the red globules, is to dissolve their colouring matter, the globule itself remaining colourless, and, according to Dr. Young, floating upon the surface.

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* Haller, Elem. Physiol. vol. i. p. 51

"This aqueous solution is of a bright red colour, and not very prone to putrefaction. When heated, it remains unaltered at temperatures below 190° or 200° Fahrenheit; at higher temperatures it becomes turbid, and deposits a pale brown sediment: if in this state it be poured upon a filter, the water passes through without colour, so that exposure to heat not only destroys the red tint, but renders the colouring matter insoluble in water."

He next describes the action of various chemical agents on the colouring matter, and then proceeds to examine how far "it is susceptible of entering into those combinations which are peculiar to other varieties of colouring matter.

"It is not a little remarkable that blood is used by the Armenian dyers, together with madder, in the preparations of their finest and most durable reds*, and that it has even been found a necessary addition to insure the perma-

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* Tooke's Russian Empire, vol. iii, p. 497.

nency of the colour*. This fact alone may be regarded as demonstrating the non-existence of iron as the colouring principle of the blood, for the compounds of that metal convert the red madder to gray and black."

In conclusion he observes, of his experiments, that "they account for the rapid reproduction of perfect blood after very copious bleedings, which is quite inexplicable upon that hypothesis which regards iron as the colouring matter, and may perhaps lead to the solution of some hitherto unexplained phenomena connected with the function of respiration. There can, I think, be little doubt that the formation of the colouring matter of the blood is connected with the removal of a portion of carbon and hydrogen from that fluid, and that its various tints are dependent upon such modifications of animal matter, and not, as some have assumed, upon the different states of oxi-

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* Aikin's Dictionary, Art. Dyeing, and Phil. Magazine, vol. xviii.

dizement of the iron which it has been supposed to contain."

Dr. Thomson remarks* of the colouring matter, that when it "is incinerated, about one-third of a per cent. of oxide of iron may be extracted from its ashes. This portion of iron is a constituent of the colouring matter, and perhaps the cause of its red colour. But in what way it is united to the albuminous portion of the colouring matter remains unknown. When incinerated, the colouring matter leaves $\frac{1}{80}$th of its weight of ashes, consisting, according to the analysis of Berzelius, of the following ingredients:

Oxide of iron	50·0
Subphosphate of iron	7·5
Phosphate of lime with traces of magnesia	6·0
Pure lime	20·0
Carbonic acid and loss	16·5
	100·0

* System of Chemistry, vol. iv. 6th edit. p. 493.

" Berzelius is of opinion that none of these bodies existed in the colouring matter; but merely their bases, iron, phosphorus, calcium, &c. and that they were formed during the incineration.

" The albumen of blood leaves the same quantity of ashes as the colouring matter. But these ashes contain no traces of iron."

I examined the arterial and venous blood of the sheep, and human venous blood, in reference to the question, whether the dried crassamentum possesses any sensibility to the magnet. The blood was dried at a temperature not exceeding 120°, and pulverised by trituration. By the most careful observation, I could not discover that the magnet possessed the smallest influence over any of the portions of blood.

The arterial blood of the sheep, incinerated and chemically examined, afforded stronger indications of iron, than either the venous blood of that animal, or human venous blood. The ash was digested in diluted muriatic acid. To this

solution ammonia was added, and a flocculent reddish coloured precipitate was thrown down. This precipitate was washed with distilled water, and re-dissolved in the least possible quantity of muriatic acid. Being then tested with precipitate of potash, a deep blue colour appeared, and the addition of tincture of galls produced a purple hue. The chemical results which I obtained would lead me to draw a middle conclusion between the opinions of Brande and Berzelius, as to the quantity of iron; but certainly the position of the former Professor, that the colouring principle of the blood is not to be referred to iron, is rendered highly probable, if not completely established.

I proceed now to enquire into the most important circumstances which influence the coagulation of the blood.

Although several of the results which will be described in the present Essay, were in part discovered by the labours of Hewson and Hunter, and have been further disclosed by succeeding physiologists, yet a more accurate investigation

of the general history of the blood seems to me to be required.

In relation to the time and manner in which the blood coagulates, and also as involving some other phenomena, I shall enter into the following considerations:—

Temperature of the blood, as affected by the air of the apartment, or by heat or cold artificially applied.

The blood placed in vacuo, and also its entire exclusion, in the first instance, from atmospherical air.

The extrication of carbonic acid gas.

Specific gravity; and dilution of the blood.

Fast or slow stream in venesection.

Rest or agitation of the blood.

Kind of vessel in which the blood is received.

The presence or absence of the fibrinous coat.

The blood as being arterial or venous.

The vital principle.

Influence of electricity and galvanism applied to the blood.

Influence of the following gases: oxygen, hydrogen, carbonic acid, azote.

Effect of some chemical agents.

The question of the evolution of heat during the coagulation of the blood.

On the fibrinous, or buffy coat of the blood.

And lastly, general conclusions and medical observations.

I must premise that, in the conducting of my experiments, it is to be understood, except when the contrary is mentioned, that the blood was taken from the human subject; that the same circumstances were observed as to the time of drawing the blood; the quantity drawn; and the kind of vessel used for its reception.

In every instance, in which I have been careful to mark with great accuracy the progress of coagulation, I have carried the blade of a pocket knife, having a point, to the bottom of the vessel, and then raising it, noticed the exact commencement of concretion.

INFLUENCE OF TEMPERATURE.

EXP. I.

Blood allowed to cool spontaneously in an apartment of medium temperature, began to coagulate at a little below 80°, and advanced much more quickly to complete coagulation, than another portion which was gradually lowered to 65°, by immersing the containing vessel in cold water.

EXP. II.

A portion of blood was kept, by means of hot water, at the temperature of 98°. I could raise a pellicle in two minutes and a half, a dense coagulum in three minutes; after which the whole mass coagulated rapidly. A second portion was exposed to the air of the apartment, 53°. At the expiration of four minutes and a half, a dense pellicle only could be raised. Even at eight minutes and a half, the general mass was semi-fluid; and uniform coagulation did not take place till eleven minutes had elapsed.

EXP. III.

Blood drawn from a person labouring under pleurisy, was kept at 120° by means of a water bath. In one minute I could raise a string of fibrin. In five minutes there was much coagulum at the bottom of the vessel, and the fibrin (for this was sizy blood) was just beginning to concrete at the surface. In seven minutes the whole mass was coagulated. Another portion was kept at a heat of 98°. In four minutes it afforded a thread ; in five minutes and twenty-five seconds, a dense coagulum at the bottom ; in seven minutes and thirty-five seconds, slight coagula throughout the mass, but the upper fibrinous portion was only beginning to concrete.

The heat of 120° was therefore the most favourable to coagulation.

EXP. IV.

Blood taken from a person in perfect health was exposed in an apartment of medium tem-

perature, and coagulated throughout in five minutes. A second portion was kept at 120°, and coagulated in less than three minutes. A third portion was subjected to the influence of a frigorific mixture, and kept at 40°. It was maintained at this degree for twenty minutes, at which period it was perfectly fluid. It was then left in the mixture without the addition of fresh ingredients; and so gradual was the process of coagulation, that it was not completed till the expiration of one hour and ten minutes*.

The several experiments were repeated with such similar results that the conclusion is clear, how remarkably a high temperature hastens the coagulation of the blood; and the converse, the retarding influence of cold, is still more strongly marked. So many other circumstances

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* Mr. Hewson observes (p. 122, vol. i.), "the blood is not coagulated (I do not mean thickened, for it is indeed thickened) by cold, but, on the contrary, has its disposition to coagulate lessened, and even entirely taken off, if the exposition to cold be long continued." This conclusion is at variance with my experiments.

besides mere temperature have an influence on the time in which the blood coagulates, that the same precise results from parallel experiments cannot be obtained; but such differences do not interfere with the establishment of the general fact.

The Blood in Vacuo, and its entire Exclusion in the first instance from Atmospherical Air.

EXP. V.

Ten cubic inches of blood, taken from a person in health, were received in two minutes. It was stirred so as to mix the whole uniformly, and then poured in equal portions into two cups.

One was placed under the air pump; the other exposed in the apartment. The blood in each cup was 81°. Immediately on the exhaustion being made, the thermometer fell to 80°; in a minute and a half, to 79·5.

The following was the comparative rate of cooling between the two portions.

Blood in vacuo.		*In the air of the apartment.*	
In 3 minutes.....	78	At the same time.....	81
— 3 ½ minutes.....	77	— 3 ½ minutes.....	80·5
— 4 minutes, a slight film on the surface	77	— 4 minutes, a slight film.....	80·5
— 5 minutes.....	76·5	— 5 minutes.....	80·5
— 6 minutes.....	76	— 6 minutes.....	80·6

At six minutes and a half we removed the blood from the air pump, and found it more coagulated than the other portion. It was considerably darker in colour.

This experiment was repeated.

EXP. VI.

Blood was taken from a man very slightly indisposed. Four ounces drawn in one minute were received in separate cups. The temperature of the blood when placed under the air pump, was 84°. In half a minute after the exhaustion, it fell to 80°.

Subsequent rate of cooling:

Blood in vacuo.		*In the air of the apartment.*	
1 minute after the exhaustion	79	At the same time	84°
2 minutes	78	2 minutes	83·5
3	77	3	83°
4	76	4	83°
5	75	5	82·5

The blood, being now removed from the air pump, was found much more coagulated than the other portion; it was free from air vesicles on the surface, the other abounding with them. It was almost black in colour, the other having the usual appearance. In nine minutes each portion was quite coagulated.

These experiments were repeated with similar results. Notwithstanding that the temperature of the blood in the exhausted receiver became so quickly reduced, it coagulated in a shorter time than the other, a result in opposition to the previous experiments as to the influence of low temperature.

Does the effect depend on the exclusion of atmospherical air, or on other causes?

I made the following experiments:

EXP. VII.

Two bottles fitted with ground stoppers were filled with blood from a glass measure, it being previously stirred. In one bottle the stopper was instantly inserted. The other bottle was left

open. Examined in five minutes, the blood in the open bottle appeared considerably coagulated, but in the other bottle it was nearly fluid. A thermometer was passed down in the centre of each column of blood, and the temperature in each was 82°. Here was a result contradicting that obtained in the experiment with the air pump. The blood excluded from the atmosphere coagulated most slowly; the temperature of the two portions being the same. I was desirous wholly to prevent all influence of atmospherical air by the following experiment.

EXP. VIII.

The jugular vein of a sheep was attached to two stopcocks, and the aperture of one was so applied to the orifice made in venesection, that the blood entered this jugular vein without having any communication with the external air. A current of blood was made to pass through the vessel, and then the lower stopcock was shut; the upper also when the vein was filled.

To this I opposed the following counter experiment.

EXP. IX.

Another portion of this person's blood, which had been received in a glass measure, was poured, after the interval of a minute from its being drawn, into another jugular vein, and which was immediately secured by a ligature.

EXP. X.

A third portion was received in a cupping glass, and exposed to the air of the apartment. Examination was made after an interval of five minutes. The blood in the cupping glass, and that poured into the vein, afforded a considerable coagulum, and in about an equal degree; but the portion which had not communicated with the external air was still fluid.

These trials were repeated with the same result; furnishing therefore the same fact as the experiment with the bottles, with stronger confirmation. In taking a review of the subject, the following perplexities appeared.

Low temperature delays the coagulation of the blood, as shown in the first experiments.

At a reduced temperature, in vacuo, coagulation is hastened.* Experiments 5 and 6.

Atmospherical air being simply excluded in the stopped bottle, the temperature not reduced, coagulation is retarded. Exp. 7.

All communication with atmospherical air being prevented, the temperature not reduced, coagulation is much retarded. Exp. 8.

It occurred to me that probably the more or less gradual extrication of the carbonic acid belonging to the blood, was the chief circumstance having an influence on the period of time in which the coagulation takes place. To put this hypothesis to the test, the following experiments were made.

EXP. XI.

A cup containing two ounces of blood drawn

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* Mr. Hewson, after remarking that cold is not the cause of the coagulation of the blood, observes that it "is therefore probably owing to the air" (p. 123, vol. i.); and in other parts of his Inquiry he evidently attributes the coagulation chiefly to the influence of the air.

from a patient ill with slight pleurisy, was instantly placed in a shallow basin containing lime water, and both were promptly introduced under a glass jar, just of sufficient height, it being desirable to admit as little as possible of atmospherical air. In four minutes the lime water was covered with a very dense pellicle ; the blood giving only a thread of fibrin.

EXP. XII.

Lime water only placed under the cover, at the end of four minutes, gave a film which was but just discoverable.

EXP. XIII.

A second portion of this person's blood, after being exposed in the apartment for three minutes and a half, was placed with the lime water. In four minutes there was a pellicle by no means so considerable as in the first experiment, in which the blood was instantly placed under the cover. From the blood itself a dense string of fibrin could be raised.

EXP. XIV.

A third portion, after previous exposure of four minutes, was placed with the lime water, and not examined till eight minutes had elapsed. The pellicle was slighter than the last, the blood advancing to coagulation.

EXP. XV.

A fourth portion was not submitted to the experiment until it had been drawn from the arm fourteen minutes, and when coagulation was almost established. In six minutes the pellicle was scarcely more than appeared in the trial with the lime water alone, Exp. 12. This blood was considerably sizy, and hence the slow coagulation.

Upon the same point of investigation, the following experiments were made.

EXP. XVI.

Blood taken from a person slightly indisposed, was drawn in a full stream. The first portion was immediately placed with the lime

water as before described. In three minutes a thick pellicle was formed, and coagulation of the blood had commenced.

EXP. XVII.

This portion was replaced with fresh lime water, and in three minutes a slight pellicle appeared, the blood being much advanced in coagulation.

EXP. XVIII.

A second portion of blood was drawn purposely in a slow stream, so that two ounces only were received in two minutes, and it was already considerably coagulated. This was placed with the lime water. In three minutes a thin pellicle only appeared over the whole surface. This portion of blood had now been drawn fifteen minutes, and was almost uniformly coagulated.

EXP. XIX.

I replaced this portion with fresh lime water,

and in three minutes its surface presented a pellicle but just visible.

EXP. XX.

From the same person, another portion was drawn in a full stream, and was instantly transferred to a basin which had been kept in a freezing mixture, made with ice and muriate of lime, producing a temperature of 28°. In a few minutes the blood was lowered to 40°, and became partly congealed. I avoided reducing it below 36°. At the expiration of twenty-five minutes, the blood, not being in the slightest degree coagulated, and its temperature 40°, was transferred to a basin made warm by means of hot water, and then placed with the lime water. In three minutes a sheet of pellicle appeared, coagulation of the blood not having commenced.

EXP. XXI.

Again transferred this portion of blood to a warm basin, which raised its temperature to 55°,

and then placed it with fresh lime water. In three minutes, a general pellicle appeared, but slighter than the last.

EXP. XXII.

With fresh lime water, a third time, using the same basin. In three minutes there was about the same degree of pellicle as in the last experiment; the blood beginning to coagulate.

EXP. XXIII.

I transferred this blood again to a warm basin, by which its temperature rose to 62°, and placed it with lime water. In three minutes a pellicle was formed considerably thicker than the last. The blood was now much advanced in coagulation.

EXP. XXIV.

Once more I repeated the trial with the lime water, and in three minutes it acquired only an extremely thin pellicle. The blood had now

been removed from the freezing mixture about sixteen minutes, and was quite coagulated.

The two last experiments appear to me very decisive; for, as in each instance only a moment of time was lost in replacing the glass cover over the fresh lime water, the carbonic acid of its included atmosphere could scarcely have had an influence, and the pellicle of carbonate of lime may fairly be referred to the agency of the blood. The pellicle in Exp. xxiii being thicker than in Exp. xxi and xxii, may be attributed to the increased temperature employed, and the advancing coagulation.

EXP. XXV.

I placed the glass cover over lime water alone, with the quick transference before mentioned, preventing as much as possible its receiving fresh atmospherical air; and in three minutes I could not detect the least film on its surface.

EXP. XXVI.

Exposing the cover for some time, and then

using it with lime water alone, I found, at the end of three minutes, little specks of carbonate here and there, but not that sheet of pellicle which the blood produces.

The following experiments appeared more clearly demonstrative.

EXP. XXVII.

A portion of blood drawn from a person in health was placed with lime water under the receiver of the air pump, which was immediately exhausted. In three minutes there appeared a thick and universal pellicle, and the blood was considerably coagulated.

EXP. XXVIII.

I made a comparative trial with lime water only, and could not in three minutes discover the slightest pellicle.

EXP. XXIX.

With a view to further illustration, a portion of the blood of a sheep, drawn from the jugular

vein, was placed with lime water under the receiver, and in three minutes, a very slight pellicle only could be seen.

As the blood of this animal coagulates in about half a minute, it became solid before it could be placed with the lime water.

Specific Gravity, and Dilution of the Blood.

From the results of a great number of experiments, I derive the following conclusions: Blood possessing the highest specific gravity, coagulates the most quickly, and the best specimens of such blood are obtained from persons in health and of robust constitution.

In blood of this description, there is the largest proportion of red particles, which are the heaviest parts of the mass. Blood which gives the fibrinous coat in a great degree, has a lower specific gravity than healthy blood, and is slowest in coagulation. Fibrin is lighter than the red particles, and also lighter than serum. In the

two most extreme examples which I have met with, the blood of a robust female in health was 1·059, and coagulated in four minutes. That of a man labouring under pneumonia, and who was bled for the fifth time, was 1·039, and did not coagulate completely till twenty minutes. I must however observe, that moderately sizy blood drawn from a person before he becomes weakened, has not commonly a very low specific gravity.

The following examination was made of the comparative specific gravity of arterial and venous blood.

EXP. XXX.

A man was bled in the vein of the arm, and in the temporal artery, on the same occasion. The specific gravity of the venous blood was 1053·5—of the arterial, 1055·5. The temperature of the venous blood as it flowed, was 96°; of the arterial, 97°.

EXP. XXXI.

Blood was taken from the jugular vein, and from the carotid artery of the same sheep. The

temperature of the venous blood was 98°, its specific gravity 1·049; temperature of the arterial blood 100°, specific gravity 1·049. The venous blood was nearer to coagulation than the arterial, as it flowed into the specific gravity bottle; and when this is considered, and the higher temperature of the arterial blood is estimated, I should be disposed to state the specific gravity of the arterial blood as rather the highest. On this occasion I examined the specific gravity of the first-drawn blood, both venous and arterial, and the last just before the death of the animal, but could not discover any difference.

I have for the most part found the specific gravity of the serum higher in proportion as the quantity of fibrin has been natural. In the most healthy specimens of blood, I have found the highest specific gravity of the serum. I may remark, that if a person be bled once or twice only, his constitution not being reduced, he will furnish blood of which the serum will possess a mean specific gravity, notwithstanding that the blood prove sizy; but, after repeated bleedings,

the specific gravity of the serum will be found low.

In the following experiments I tried the effect of dilution of the blood with water:

EXP. XXXII.

One part of blood of healthy appearance, was mixed with three parts of water warmed to 80°. In fourteen minutes it had acquired a jelly consistence. As it advanced to further coagulation, its texture was very loose in comparison with the crassamentum of blood alone. In the counter experiment with a portion of the same blood in its natural state, the coagulation was firm in five minutes.

EXP. XXXIII.

Blood of the same character, was mixed with four parts of warm water (80°), and in nine minutes it acquired the consistence of a tremulous jelly. A portion of this blood alone became solid in five minutes.

Mr. Hunter obtained different results, as he

states (p. 98), "that water rather hastened coagulation, but made it neither firmer nor looser in the texture."

Fast or slow Stream in Venesection.

EXP. XXXIV.

Three ounces of blood were drawn from the arm in two minutes. In five minutes, its temperature being 77°, it afforded only a dense pellicle.

EXP. XXXV.

A second portion was drawn in a slow stream, so that two ounces and a half only were received in three minutes and a half. In one minute and a half, this blood, its temperature 80°, was almost wholly coagulated. I repeated the experiment several times, with a similar result. I inferred, that the blood passing down the arm in so slow a manner, parts with its carbonic acid more readily, and more completely.

In watching the coagulation of the blood of the sheep, I have found that the first-drawn blood, whether arterial or venous, coagulates with remarkable rapidity as compared with human blood; but the blood which passes from the vessels when the animal is dying, is almost instantaneously solid. Blood drawn from a person about to faint, coagulates very rapidly.

Rest or Agitation of the Blood.

It seemed probable that agitation of the fresh-drawn blood for a short time, would accelerate the escape of the carbonic acid, and in that way favour the coagulation. The following experiments supported the correctness of the idea.

EXP. XXXVI.

Blood of healthy appearance was received in a glass measure, and, being mixed, was poured in equal portions into two cups. The one was left at rest. The blood in the other cup was stirred

for two minutes, and coagulated one minute sooner than the other portion. In temperature it lost half a degree. The portion which had not been disturbed, coagulated in six minutes.

EXP. XXXVII.

This experiment was repeated precisely in the same manner, and with a result exactly similar.

If the blood be stirred for a considerable time, the fibrin becomes separated from the red particles, and the ordinary mode of coagulation is prevented, as shown in the following experiment.

EXP. XXXVIII.

The blood, immediately after being received in the basin, was kept briskly stirred with a piece of stick, and this being continued for a quarter of an hour, a mass of fibrin became twisted around it. The coloured portion in the basin remained permanently fluid; and from subsequent digestion of it in water, no more fibrin could be obtained. Hence we see, in a simple and familiar manner, that the solidification of

the fibrin is the essential cause of the coagulation of the crassamentum of the blood. This fact of the separation of the fibrin by stirring the blood, is mentioned by Mr. Hewson.

Kind of Vessel in which the Blood is received.

The time in which the coagulation of the blood is effected, differs very much according to the kind of vessel into which it is received.

EXP. XXXIX.

The vessels employed were, a tea cup, a saucer, and an open-mouthed bottle holding two ounces. In three minutes I could raise a thread of fibrin from the bottle, not until five minutes in the same degree from the cup, and not till seven from the saucer. In the same ratio, the coagulation proceeded. At the end of nine minutes the temperature of the blood in the bottle was 75°, in the cup 70°, in the saucer 60°. I repeated this experiment with different vessels

several times, and found the coagulation most delayed when the blood was received in a pewter porringer, from the obvious circumstance that this vessel is a good conductor of heat.

EXP. XL.

I was desirous to determine, by the following experiment, whether the form of the vessel would influence the time of the coagulation independently of temperature.

EXP. XLI.

Blood was received in a glass measure, and, being gently stirred, was poured in equal portions into a tea cup and a saucer. Both these vessels were kept immersed in a basin of hot water. The blood in each coagulated exactly and equally in four minutes, the temperature of the blood being then 94°.

The Presence or Absence of the Fibrinous Coat.

I have already mentioned that blood drawn from a person in health, coagulates very quickly; and unless there should be the agency of cold to delay the process, I may state as the range of time, from four to seven minutes for the blood to become a firm jelly. Blood which possesses the fibrinous coat requires the most time for its coagulation. I shall relate the following example:

EXP. XLII.

The patient was dangerously ill with pneumonia. The different portions of blood were received in small basins. The first portion had the appearance of being quite fluid at the end of seven minutes, and had the usual blueish colour of sizy blood. I carried the handle of a teaspoon to the bottom of the basin, and could raise a coagulum. At the expiration of eleven minutes, the coloured portion of the blood was quite coagulated, although the upper portion continued

fluid. Here we have to observe, that the coagulation, though slow, is not so much delayed as we should conclude, if confining our examination to the upper part of the vessel.

EXP. XLIII.

Another portion of this blood which had not been disturbed, was examined at the end of nine minutes and a half. It was just concreted at the surface, so that the finger did not receive from it any moisture; but on breaking this thin coat with a tea-spoon, the serum, or rather serum mixed with some fibrin yet fluid, escaped freely; and next, examining at the bottom of the vessel, I found the coloured blood quite coagulated. In this experiment a familiar proof is presented of the range of specific gravity in the component parts of the blood: crassamentum or the red particles, blended with fibrin, being the heaviest; serum the next in density; and fibrin, in admixture with serum, the lightest. The order of coagulation also, according to the presence or absence of the fibrinous coat, is worthy of observa-

tion. In healthy blood, as I shall presently describe, coagulation commences in the middle stratum, where the temperature is the highest. The blood first received in the vessel is most cooled and remains at the bottom. The fibrin, as I shall show in a future experiment, mixing itself with the red particles, arranges itself very equally throughout the mass. In reference to the former experiments which I have stated, I conclude that the carbonic acid escapes the soonest from that portion of the blood which possesses the highest temperature; and where, as it appears to me, coagulation first takes place.

EXP. XLIV.

Blood was received in an open-mouthed bottle in order to have an equal column. The general mass became nearly coagulated in seven minutes, when the temperature at the top was 78°, in the middle 80°, and at the bottom 77°; and certainly the blood remained fluid for the greatest space of time at the bottom of the bottle. I satisfied myself also that the coagulation com-

menced in the middle. But, as just now related, the order of coagulation in sizy blood, is different. It commences towards the bottom of the vessel, because the fibrin is in excess, and that which is blended with the red particles, concretes much more readily than that which is mixed with the serum.

The Blood as being arterial or venous.

EXP. XLV.

A man suffering from ophthalmia, was bled in the arm, and in the temporal artery, in immediate succession. The blood was received in cups previously warmed by water, of the temperature 96°. The bulb of the thermometer was held in the stream of blood from vein and artery. By that of the vein, it was raised to 96°; of the artery, to 97°. The arterial blood coagulated in rather less time than the venous. A given quantity of blood was received from the vein in less time than from the artery, the stream being much

larger. Therefore the carbonic acid could pass off more readily from the arterial blood.

EXP. XLVI.

Blood was taken from the jugular vein, and from the carotid artery of the same sheep. The temperature of the venous blood was 98°;* of the arterial, 100°. The venous blood coagulated in rather the shortest time; the flow of blood being slower than from the artery; the reverse of what happened in the last experiment.

EXP. XLVII.

In another experiment with the sheep, I found that the venous blood was firmly coagulated in

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* On the day on which this experiment was made, the cold in the slaughter-house reduced the thermometer almost to 32°. On another occasion, the day being warm, I found the temperature of the blood issuing from the jugular vein of the sheep, as high as 104°. I do not intend to state, that there was this real difference of 98° and 104°. It is to be considered that it is very difficult to estimate the true temperature of the blood as it flows from the orifice, and in a cold atmosphere.

half a minute, and equally as the arterial blood in a minute and a quarter.

The Vital Principle.

EXP. XLVIII.

This blood was taken from a man in health. A portion of the small intestine of a young rabbit killed on the previous day, and a similar portion from a rabbit killed purposely at the moment of the experiment, were used as the recipient vessels of the blood. They were armed with double stop-cocks, and the orifice of one was applied to the vein, so that the blood entered direct, as described in Experiment viii. The blood received in the intestine of the newly killed animal, gave a small coagulum in eight minutes and a half, and was rather more advanced than the blood received in the other intestine, which I thought might be attributed to the higher temperature of the intestine just taken warm from the body. Each being examined at the expiration of four-

teen minutes and a half, I found that the intestine from the newly killed animal contained much the largest quantity of coagulum.

EXP. XLIX.

Another portion of this blood received in a cup, and exposed in the apartment, coagulated entirely in eight minutes.

With a view to further illustration, the following experiments were made with the blood of the sheep, conducted in the same manner, except that the jugular veins of the sheep were employed as the recipient vessels:

EXP. L.

The blood in the vein taken from the animal killed on the previous day, afforded a slight coagulum in four minutes and a half, and in another minute a firm coagulum. The other vein examined at the end of ten minutes, was completely occupied with coagulum.

EXP. LI.

In a second experiment, the blood in the vein taken from the newly killed animal, was

examined at the end of five minutes, and was firmly coagulated.

On another occasion, when the same experiments were made in the slaughter-house, where the cold of the atmosphere was almost 32°, the coagulation of the blood did not take place in so short a time; and when we consider the thin covering given by the jugular vein, we must attribute much to the influence of high and low temperature of the atmosphere. From the difference of covering given by the intestine and the vein in these experiments, I conceive that the longer comparative period required for coagulation in the one than the other, is to be explained. The carbonic acid would more easily escape through the thin coats of the jugular vein.

If blood be confined by ligatures in the vessels of the living animal, it remains fluid for a considerable time. Mr. Hewson made several experiments on this point, tying up the jugular veins of the dog, and examining the state of the blood at different intervals. He remarks, "So very slowly does this coagulation proceed, that

in an experiment where I had the curiosity to compare more exactly the clotted part with the unclotted, I found, after the vein had been tied two hours and a quarter, that the coagulum weighed only two grains, whilst the rest of the blood, which was fluid, on being suffered to congeal, weighed eleven grains." This author considers that the blood of the dog begins to coagulate in three or four minutes, and is completed in seven or eight. I have found that the blood of this animal coagulates in a much shorter time :— in one instance, the blood being taken from the jugular vein of a puppy, the whole mass was solid in three minutes.

The following experiments were made upon the blood of the horse, to illustrate the influence of the living vessel not yet removed from the animal.

An inch and a half in length of the jugular vein was included between two ligatures ; and at the end of an hour, was emptied of its contained blood, which was quite fluid, but which coagulated in five minutes after, being received in a cup.

The experiment was repeated, and at the expiration of one hour and three quarters, the blood was fluid, but coagulated in the cup in which it was received, in five minutes.

A similar experiment was made on the state of the blood in the vein immediately dissected away and removed from the animal, after the ligatures were passed. At the interval of forty minutes the blood was thickened, but not yet coagulated. The atmosphere was at a low temperature.

In reference to Mr. Hunter's opinion on the subject, I was desirous to ascertain whether the blood would lose the property of coagulating, in the case of sudden destruction of life by electricity.

EXP. LII.

A strong mongrel dog was the subject of the experiment. By means of a very powerful electrical battery*, constructed by Mr. Woodward at

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* A flatted steel wire (commonly called watch pendulum wire,) 7½ feet in length, was completely fused and dispersed

the Surrey Institution, the animal was killed after several shocks. On inspection next morning, the blood in the cavities of the heart, and in the vena cava, was found quite coagulated.

I made a similar experiment with rabbits, with the same result; and in these smaller animals, death was effected instantaneously.

Influence of Electricity applied to the Blood.

EXP. LIII.

The whole of the blood drawn, and which was taken from a man very slightly indisposed, was mixed, and then poured in equal portions into two cupping glasses. The temperature of the blood at the moment before the machine was put in action, was 85°. The chain was put in the blood. Instantly the thermometer rose to 86·5. The other cupping glass was placed on the table. The two portions of blood cooled in the following ratio:

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in red hot balls, by a discharge of the batteries, when the electrometer indicated only fifty degrees of intensity.

The electrified blood.		*The other portion of blood.*	
In 3 minutes was.....	84·5	In 3 minutes was.....	83°
— 4 minutes.....	83·5	— 4 minntes.....	82°

Afterwards, each portion cooled in equal time; and each was considerably coagulated in five minutes; but the electrified blood was rather the most firm.

The elevation of temperature from the electricity appearing to be a remarkable circumstance, I shall relate some of the additional experiments which were made.

EXP. LIV.

The blood was taken from a female in health. The experiment was conducted as before. In one minute, each portion was almost coagulated, but the electrified blood received an immediate elevation of two degrees of temperature. After three minutes, the thermometer in each blood stood at 83°, the electrified portion having fallen from 86 to 83°, the other from 84 to 83°. The rapid coagulation of the blood in this instance, was owing to the accidental circumstance of using two cupping glasses which had only just

been emptied of warm blood ; but they were of the same temperature.

EXP. LV.

In this instance the electrified blood immediately gained two degrees. The rate of coagulation appeared to be very equal, but the electrified portion was certainly the most compact in texture.

Of Galvanism.

EXP. LVI.

Healthy blood was portioned into two cupping glasses. In that exposed to the air of the apartment, a pellicle could be raised in four minutes ; and, in six, a dense coagulum.

Thirty pair of plates, four inches square, were employed for the galvanic action. Immediately on introducing the wires, at the negative one, a mottled scum appeared, having, in colour, shades of green, red, and yellow, with a copious disengagement of gas. To the positive wire there

was attached a dense black coagulum, which, as it dried, assumed the appearance of charcoal. At the expiration of ten minutes, each portion of blood was quite coagulated, and the temperature of each was 76°. The change of temperature in the galvanised blood was not noticed in the first instance.

EXP. LVII.

This experiment was repeated. The temperature of the apartment was 46°. The patient was a man complaining only of a cold. As in the former instance, thirty pair of plates were used. In one minute after the application of the wires, the temperature of the blood became 78°; that of the portion in the apartment 74°; and the following was the progressive and comparative rate of cooling:

	Galvanised blood.	*Other portion.*
In two minutes.....	74·25.....	72·25
— four..	72·25.....	71
— six.....	72	70
— eight.....	71	69

In three minutes the galvanised blood afforded

a pellicle, when the other portion had not begun to coagulate. A similar difference continued; and at the expiration of nine minutes, the galvanised blood was very much advanced in coagulation; and the other gave only a dense coagulum from the bottom.

Of Oxygen Gas.

EXP. LVIII.

The blood was drawn from a man who had received a slight accident. It was divided into two cupping glasses. The one was placed under the air pump, and, after exhaustion, oxygen was introduced*. At the same moment of time the

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* Mr. Garden of Oxford Street, who rendered me his obliging assistance in many of these experiments, contrived an ingenious apparatus for the introduction of the gases, of which the following is a description:

A glass receiver, open at top and bottom, and having also a lateral opening, furnished with a brass cap and stopcock, for the purpose of introducing any gas after the atmospheric air had been extracted.

The superior opening of the receiver was closed by a

other cupping glass was placed under another air pump, aud, after exhaustion, atmospherical air was again admitted. This was done in order that circumstances should be alike as to the agitation of the blood from the working of the machine. The blood under the influence of the oxygen coagulated the soonest. In eight minutes it afforded a dense coagulum ; the other blood, only a dense pellicle. But it is more particularly worthy of observation, that at the interval of eight minutes from the beginning of the experiment, the temperature of the blood exposed to oxygen was 85°, and of the other portion only 77°.

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brass plate, through which a brass wire was made to pass, moveable in collars of leather, and having its lower extremity terminated by a curved and rather broad piece of metal, so that upon plunging it into the blood, the state of coagulation of that fluid might at any time be ascertained. A bladder, containing the gas intended for experiment, was connected with the stopcock of the lateral opening, and when the receiver was exhausted upon the air pump, the subsequent opening of the stopcock permitted the immediate introduction of the gas.

EXP. LIX.

In another experiment, conducted exactly in the same manner as relating to the oxygen, but in which the other portion of blood was merely exposed upon the table, the temperature of the oxygen blood was 77°, of the other 75°. These two experiments support each other in the fact, that the chemical influence of the oxygen produces a considerable evolution of free caloric. It must be kept in recollection that in the Experiments v and vi, in which the comparison was made between blood placed in vacuo, and in the atmosphere of the room, the reduction took place in the one to the extent of four degrees and a half in one instance, and seven degrees and a half in the other; in which last I may observe the exhaustion was more complete.

Of Hydrogen.

EXP. LX.

The patient, a female, having a slight cough, but the pulse natural. Two cupping glasses

were used. One was placed under the air pump, the other exposed as usual. After the exhaustion, hydrogen was introduced. From each blood I could raise a thread of fibrin in one minute and a half, and a dense coagulum in four minutes. The coagulation of the mass afterwards proceeded rapidly, but was completed in rather the least time in the hydrogen blood. At the expiration of nine minutes, the blood in the atmosphere of the room was 78°; but in the portion taken from the hydrogen, only 75°; and also it appeared less florid than the other blood.

Of Carbonic Acid Gas.

EXP. LXI.

This experiment was conducted in the same manner as the last. In three minutes, coagulation commenced in the blood exposed to the air of the apartment: in four minutes, in the blood communicating with the gas. In eight minutes the blood was removed from the air pump,

when it was not so much coagulated as the other portion. Its temperature was then 70°, and the blood exposed to the apartment was as high as 79°. This is a very remarkable difference, and presents a striking contrast to the effect produced by oxygen gas.

Of Azote.

EXP. LXII.

The patient was a man labouring under aneurism of the aorta. His pulse was 120, and throbbing. This proved to be sizy blood. In the same space of time that I could raise a pellicle only from the blood under the influence of the azote, I could obtain a dense coagulum from that portion exposed to the air of the room. Finally, the blood affected by the gas had a very livid appearance, and was looser in its texture than the other portion. In consequence of an accident with the thermometer, the comparative temperature could not be noticed.

EXP. LXIII.

Temperature of the apartment 46°. Blood taken from a woman but slightly indisposed, was received in a glass measure, and after being stirred, was divided into equal portions. One was subjected to the influence of azote; the other was placed in the apartment; and the following comparison was made. It appeared that the introduction of the gas raised the temperature of the blood; but we could not rely on the comparative experiment, as the day was severely cold, and the gallipot, containing the other portion of blood, was carelessly placed in a current of cold air entering from the other apartment in which there was not a fire. It is certain, however, that the blood exposed to the azote coagulated about two minutes sooner than the other; and this difference must be attributed to the low temperature of the blood exposed to the cold air.

Effect of some Chemical Agents.

Mr. Hewson, in his Experimental Inquiry,

p. 12 and 13, has classed the salts which, he states, " keep the blood fluid by itself, and yet allow it afterwards to jelly when mixed with water;" and those which, he says, " likewise keep the blood fluid, but do not allow it to jelly when mixed with water." I obtained the following results, which, in some instances, differ from those recorded by Mr. Hewson.

EXP. LXIV.

A saturated solution of sulphate of soda added immediately to blood drawn from the arm, does not altogether prevent coagulation. A weaker solution renders it a soft uniform jelly, and of a florid red colour.

EXP. LXV.

A saturated solution of sulphate of potash, produces a weak uniform coagulation; the colour florid red.

EXP. LXVI.

A saturated solution of borax produces the same appearances.

EXP. LXVI.

A saturated solution of tartrate of soda prevents coagulation, but the blood thickens; and if a weaker solution be used, coagulation, of a soft jelly consistence, takes place.

EXP. LXVII.

A saturated solution of common salt keeps the blood fluid, and of a dark colour. With a weak solution it coagulates loosely.

EXP. LXVIII.

Muriate of Ammonia produces the same appearances.

EXP. LXIX.

A saturated solution of nitrate of potash keeps the blood fluid. A weaker solution allows of loose coagulation. The colour florid red.

EXP. LXX.

Diluted muriatic acid, and diluted sulphuric acid, each equally produced a dark pitch-like colour of the blood.

EXP. LXXI.

The medicinal prussic acid did not appear to delay coagulation.

EXP. LXXII.

By the addition of extract of belladonna, the fibrin formed into a firm clot.

EXP. LXXIII.

The liquor potassæ of the Pharmacopœia was added to fresh drawn blood, which was of healthy aspect, in the proportion of twenty minims to one ounce of blood. The blood was rendered permanently fluid. It is to be concluded that the alkali acts as a solvent of the fibrin.

EXP. LXXIV.

Five grains of sulphate of zinc, dissolved in one ounce of distilled water, produced a thickening of the blood, but not coagulation.

EXP. LXXV.

Sulphate of copper in the same proportion, produced rather more effect — a more dense precipitation.

EXP. LXXVI.

Sulphate of alum in the same proportion, produced rather less effect than the zinc.

EXP. LXXVII.

Nitric acid diluted with seven parts of water, produced a very dense precipitation. The blood remained dark in colour.

EXP. LXXVIII.

A saturated solution of the three salts last mentioned was added to fresh-drawn blood. The solution of alum, instantly produced very dense and tenacious coagula ; but the other solutions, a grumous thickness of the blood. The sulphate of copper had more effect than the zinc.

On the Question as to the Evolution of Heat during the Coagulation of the Blood.

The late lamented Dr. Gordon, of Edinburgh, having arrived at a different conclusion from that formed by Mr. Hunter and Dr. John Davy, on the present question, I have been induced to make some further experiments, with every care in my power to avoid error, and an attempt to explain the contrary results obtained by those distinguished physiologists.

EXP. I.

Blood was received in a wide-mouthed bottle, about six ounces in capacity; and instantly on its being filled, the bulb of a delicate thermometer passing through a cork which was promptly fixed, was inserted in the centre of the column of blood. The bottle was kept carefully at rest, and the state of the thermometer was noticed with unremitting attention. The following was

the progressive rate of cooling from its first temperature 91°.

In one minute.....90°
— two minutes.....89·50
— three.....89°
— four.....89°
— five.....89°
— six.....89°
— seven.....88·50
— eight.....88°
— nine.....87·75
— ten.....87·50

The thermometer was now removed, and the blood was found coagulated, and of healthy appearance.

EXP. II.

Two thermometers, precisely alike in sensibility, were employed in this experiment, the bulb of one being carried almost to the bottom, and of the other made to rest rather more than two inches higher in the column of blood, and about an inch below the surface.

Previously to the experiment, the bottle was warmed by filling it with water of 95 degrees of heat.

The following was the comparative rate of cooling in the upper and lower part of the blood.

	Upper Thermometer.	Lower Thermometer.
At first	92°	92°
In half a minute	93°	92·50
Ditto	93°	92·50
Two minutes	93°	92·50
Ditto and a half	92·50	92·75
Three minutes	92.50	92°
Ditto and a half	92·50	92°
Four minutes	92·15	91·75
Five	91·75	91°
Six	91°	91°
Seven	90·75	90·50
Eight	90·25	90°

The blood was quite coagulated, and had a healthy appearance.

EXP. III.

The experiment was repeated in the same manner as the last.

	Upper Thermometer.	Lower Thermometer.
At first	94°	94°
In one minute	95°	95°
— two minutes	95·50	95°
— three	95°	94°
— four	95°	94°
— five	94·50	93°
— six	94°	92°
— seven	93·50	91°
— eight	93°	90·50
— nine	92°	90°

The blood at this time was firmly coagulated, and appeared healthy.

EXP. IV.

The Experiment was once more repeated.

	Upper Thermometer.	Lower Thermometer.
At first	94°	94°
In half a minute	95°	95·50

	Upper Thermometer.	Lower Thermometer.
— one minute	95°	95·75
— two minutes	95°	95·75
— three	94·75	95·50
— four	94°	95°
— five	93°	94·50
— six	93°	94°

The blood was quite coagulated, and, as in the other instances, was healthy in appearance. The four patients were so slightly indisposed, as to be favourable subjects for the occasion.

EXP. V.

A robust young man, in perfect health, was bled purposely for the sake of the Experiment. One thermometer only was used, and the bottle was not previously warmed.

The temperature of the blood was stationary for three minutes at 93°, and then suddenly rose to 94°, after which the rate of cooling was as follows:

At the expiration of five minutes and thirty seconds.....93°

6 minutes and 20 seconds.....92°

8 minutes.....92°

10 do.....90°

11 do.....89°

12 do. and 30 seconds.....88°

The blood was of jelly consistence in eight minutes, and firmly coagulated in ten.

EXP. VI.

This was an example of remarkably sizy blood. I immersed the bulb of the thermometer in the mixed serum and fibrin, so as to cover it completely, and held it there steadily. The temperature was raised from 80° to 81° just at the moment that the concretion of the fibrin began. For a short time before it had been stationary, and afterwards, it continued exactly at 80° for the space of five minutes.

EXP. VII.

Blood drawn from a person in health, was received in a glass bottle into which the stopper was inserted as soon as it was filled. At the ex-

piration of seven minutes it was considerably coagulated, and being now examined by the thermometer, it was found, just below the surface 78°, in the middle 80°, at the bottom 77°.

EXP. VIII.

A mixture of warm water and starch, the specific gravity of which was 1·050, was examined by two thermometers in the same manner as the blood; the bottle being previously warmed.

	Upper Thermometer.	Lower Thermometer.
At first	91°	92°
In two minutes	91°	92°
— three	91°	92°
— four	90°	91·50
— five	89·50	91°
— six	89°	90·25
— seven	88·50	90°
— eight	88°	89·25
— nine	87·75	89°
— ten	87°	88·25
— eleven	87°	88°

EXP. IX.

The same Experiment was made with water only.

	Upper Thermometer.	Lower Thermometer.
At first	95·50	95·50
In one minute	95°	94·50
— two minutes	94·50	93·75
— three	94°	93°
— four	93·50	92·25
— five	93°	91·50
— six	92·50	90·75
— seven	91·50	90°

From this series of experiments I am led to the conclusion, that a slight evolution of heat takes place during the coagulation of the blood. I have recited several experiments, because, as the blood varies more or less in every example, it appears to me important to demonstrate the difference of results obtained, as well as those most favourable to my opinion.

Experiment i serves to show the probability that heat is given out from the blood in the progress of coagulation; as, in the long interval of six minutes, it lost only one degree of temperature; the reduction being from 90° to 89°.

Let this be contrasted with the result of the Experiments made with starch and water, the density of which mixture was almost equal to the average and specific gravity of blood. In six minutes it lost two degrees of heat at the upper parts, and almost two at the lower. In the Experiment with water, the loss in six minutes was, in the upper part of the bottle, three degrees, and in the lower almost five. This difference between the temperature of the upper and lower part of these two fluids may be ascribed to the greater tenuity of the water, by which the colder particles would more readily descend, than in the case of the viscid mixture of starch and water.

In Experiment ii, the bottle having been previously warmed, the reduction in six minutes, both by the upper and lower thermometer, was from 92° to 91°; but here we have the fact that,

at the interval of two minutes and a half, there appeared an actual slight elevation of temperature in the lower thermometer, in which fact it was impossible to be mistaken, as the progress of the experiment was most accurately watched. The rising of the mercury in the first half minute is merely to be considered as its adjustment to the heat of the blood.

Experiment iii affords a similar demonstration, with the only difference that the slight increase of temperature was marked by the upper thermometer, and at the interval of two minutes instead of two minutes and a half. The warm water had not been kept in the bottle for so long a time as in the previous experiments, on which account the bottom might be rather cooler than the upper part, and abstract more of the heat of the blood. I may also observe that unless the bottle should throughout receive a temperature equal to that of the blood, the portion first entering the bottle would be most cooled.

In Experiment iv the warm water was kept in the bottle for a longer time, and, as in Experi-

ment ii, the lower thermometer showed the highest temperature. At the interval of one minute the slight evolution of heat was marked by the lower thermometer. Experiment v sets forth the fact which I have endeavoured to prove in the most unequivocal manner. In this case the bottle was not previously warmed, and one thermometer only was used. After the interval of three minutes there was an actual elevation of one degree; a result quite free from all ambiguity. Blood drawn from a healthy and robust person is the most favourable for the experiments, because such kind of blood coagulates most quickly. It is of consequence that the bottle should be of large capacity in its diameter, so as to allow a greater body of coagulum.

Experiment vi confirms the last result in the most striking manner, and enabled me to observe that the elevation of the thermometer, which in this instance was one degree, took place just at the period when the fibrin began to concrete.

Experiment vii was made for the purpose merely of showing the rate of cooling in different

parts of the column of blood, the bottle in which it was received not having been warmed: and of the results of this experiment I shall presently take further notice. Mr. Hunter inferred from his experiments that no heat was given out during the coagulation of the blood. He chose a turtle as the subject of his most conclusive experiments. He makes the following statement: "The animal being suspended by the hind legs, the head was cut off at once, and the blood caught in a basin. The blood while flowing was 65°, and when collected was 66°, but fell to 65° while coagulating, which it did very slowly; it remained at 65°, and when coagulated was still 65°*."

The permanency with which the blood in this experiment maintained the temperature of 65°, is to be attributed to the temperature of the atmosphere, which Mr. Hunter maintains was also 65°; but how is it explained that the elevation to 66° should have been attained, if not derived from

* A Treatise on the Blood, &c. p. 28.

a change of capacity in the blood itself while coagulating? The most favourable moment for finding the highest temperature of the fluid blood was at the time of the stream flowing from the animal.

Dr. John Davy has recorded his chief experiments on the present question in his Inaugural Dissertation, published at Edinburgh, in 1814. He chose the blood of the lamb. The temperature fell from 103·50 to 101·75 in five minutes. The bottle had been stopped as soon as filled, and the bulb of the thermometer was inserted in the middle.

In the second experiment the bulb was kept near the surface, and the bottle was left open to the atmosphere, the temperature of which was 67°. For four minutes the heat of the blood was steady at 103°, and in 5 minutes it had only lost a quarter of a degree. To these results Dr. Davy contrasted the cooling of water, the temperature of which for three minutes was stationary; in four, lost a degree; in five, a whole degree. These differences are not so remarkable

as those which I have found. The last experiment, however, with the blood, appears in support of my opinion. It seems to me that the high temperature of the blood of this animal must render it much less favourable for the experiment than human blood; for as it coagulates in less than a minute, there is scarcely an opportunity afforded for the accurate examination of the phenomenon in question. Dr. Davy suggests that in the experiments both with the blood and with water, the steadiness of the thermometer might be owing to the upper contents of the bottle deriving heat from the lower. It was in reference to this opinion that I employed two thermometers in several of my Experiments, and warmed the bottle previously. In experiment vii it appears that the lowest temperature was at the bottom, and this effect must happen invariably, unless the bottle is previously warmed The blood first received is most cooled by the bottle, and being so dense a fluid, it remains at the bot-

tom not displaced by the portion fresh flowing in.

Evaporation must take place most freely near the surface, and hence it follows that the middle of the column is the warmest part. Dr. Gordon, in a paper in the Annals of Philosophy, vol. iv, has, it appears to me, most erroneously founded his opinion as to the sensible evolution of heat during the coagulation of the blood, on the circumstance of finding the thermometer rise considerably by bringing the bulb from the bottom towards the surface, where coagulation had made most progress. I have just now offered the explanation of this fact.

Dr. Davy replied to Dr. Gordon in the fourth No. of the Journal of Science and the Arts, relating further experiments on the blood of the turtle, the shark, and the sheep, maintaining his former opinion and supporting it by the following explanation, with a view to reconcile the seeming deviation from this general principle: "That change of temperature is a necessary consequence of the change of form of bodies in general." He re-

marks "Since, during the coagulation of blood, a part of it passes from the liquid into the solid state, there should be, according to theory, some increase of temperature. But since this liquid part, the fibrin, which becomes solid, is so small as to amount only to about $\frac{1}{30}$ of the whole quantity by weight; and since the coagulation is not an instantaneous but a slow and gradual effect; it appears to me as necessarily to follow, that the heat produced must be too slight to affect sensibly the thermometer. This granted, which may be proved to demonstration, the anomaly vanishes—this fact no longer opposes the general principle*".

In answer to these ingenious observations, I venture to oppose the results which I have obtained, and of the accuracy of which I am quite satisfied. It appears to me that the most remarkable evolution of sensible heat takes place just at the moment of time when the fibrin first concretes; but also the long pause of the thermo-

* Journal of Science and the Arts, No. iv, p. 248.

meter, so much exceeding that which happens with the other fluids, surely does demonstrate that a slight extrication of heat continues throughout the progress of coagulation. We have to estimate not merely the distinct influence of the solidification of the fibrin, but also that of the combined effect of the whole of those materials of the blood which enter into the formation of the clot, commonly called the crassamentum.

Of the Fibrinous or Buffy Coat of the Blood.

I here wish to offer a few preliminary remarks, before I detail the experiments on this part of my inquiry.

If blood be received in a slow stream, and consequently its coagulation take place quickly, (see Exp. xxxv) notwithstanding that it may be rather sizy in its nature, it will fail to exhibit the fibrinous coat which would appear if the blood were drawn in a fast stream. This fact of hastened coagulation interfering with the formation

of the fibrinous coat is still more strongly exemplified if the blood be received in a vessel purposely warmed, for the reason that coagulation takes place much more quickly. If sizy blood be stirred for a short time, the fibrinous coat becomes much less evident than in another portion left undisturbed. In the case of the slow stream and hastened coagulation, I conceive the explanation to be, that sufficient time is not afforded for the due separation of the component parts of the blood in the order of their specific gravity. In the example of the blood being stirred, not only is the coagulation rather hastened, in consequence, as I conclude, of the accelerated escape of the carbonic acid, but I apprehend that the agitation causes more of the fibrin to be blended with the red particles than if the mass were left more to the spontaneous separation of its parts. Hence it happens that when the excess of fibrin beyond the healthy proportion is only slight, the stirring of the blood for a minute or two will prevent the appearance of the buffy coat, a circumstance mentioned by Sydenham; but if the ex-

cess of fibrin be very large, the buffy coat will be formed, notwithstanding the disturbance.

As medical diagnosis and treatment are, in many cases of disease, founded in a great measure on the appearance of the blood drawn from the arm, it becomes of great importance to establish our conclusions on the basis of careful experiment and observation. I have used the terms, sizy blood, and buffy, and fibrinous coat, in a synonymous sense. Blood having this character is called, in still more familiar language, inflamed. With what propriety I shall afterwards consider.

The following was the method adopted in the experiments which I have to relate:

The clot of blood was drained of its serum by being put into a linen bag. It was then weighed, and also the whole of the serum was weighed, unless from accident some portion had been lost. The clot was then tied up in a linen bag, and being held under a fast stream of water, was gently kneaded with the finger, so that it was made to part with all its soluble material. Being thus washed till the red particles were quite re-

moved, the firm colourless residuum was dried at 212°, and afterwards weighed. The weight of this, the fibrin, being ascertained, the calculation was made for its proportion to 1000 grains of the moist clot: but being desirous also to ascertain the usual loss which the moist clot would undergo from a temperature of 212°, I made six experiments for this purpose, and found the mean result of loss to be $\frac{3}{4}$. It is obvious, therefore, that if the reader prefer to consider the relation of dry fibrin to dry instead of moist clot, he will read for 1000 grains which he finds stated, 250 grains; and, as a standard drawn from six Experiments, it may perhaps be fairly allowed, an approximation to the truth.

In what order does the fibrin arrange itself in healthy blood?

EXP. LXVI.

Nine ounces of blood were taken from a man in health, and received in a cylindrical glass jar. The vessel was kept carefully free from agitation, and the blood allowed to coagulate in a medium

temperature. On the following day the whole column was divided into three equal portions by transverse sections, and equal weights being taken, the fibrin of each portion was separated and dried at 212°.

The proportions for 1000 grains of clot were, for the upper section, $3\frac{1}{2}$ grains, the middle $3\frac{1}{4}$ grains, the lower 3 grains.

EXP. LXVII.

This Experiment was repeated on the blood of a man who had experienced a fall the day before, but he did not complain of illness.

The total quantity of clot was 2802 grains, of serum 1551 grains, and the specific gravity of the serum at 60° was 1·031. The proportion of fibrin to 1000 grains of clot was, for the upper section, 4·28 grains, the middle 3·74 grains, and the lower 4·28 grains.

I proceed to offer examples of the proportions of fibrin in various kinds of blood, describing such collateral circumstances regarding either the blood or the patient, as it proved convenient to notice.

EXP. LXVIII.

Blood free from fibrinous coat. The clot has thin edges and is loose in texture.

Of Clot.	Dry Fibrin.	Sp. gr. of the serum.
1000 grs.	4·25 grs.	1·026.

EXP. LXIX.

Blood similar in appearance to the last, but its texture not quite so loose.

Clot.	Dry Fibrin.	Sp. gr. of the serum.
1000 grs.	4·41 grs.	1·026.

EXP. LXX.

A compact cupped clot, thick converging edges, a fibrinous coat, but not thick, as the coloured clot is seen through the buff.

Clot.	Dry Fibrin.
1000 grs.	12·41 grs.

EXP. LXXI.

Blood of the same character as the last.

Clot.	Dry Fibrin.
1000 grs.	13·73 grs.

EXP. LXXII.

Clot of loose texture, and even, thin edges.

Clot.	Dry Fibrin.
1000 grs.	3·03 grs.

EXP. LXXIII.

Blood taken from a man who had been bled very repeatedly, having an aneurism of the aorta. It had a thin fibrinous coat.

Clot.	Dry Fibrin.
1000 grs.	7·52 grs.

EXP. LXXIV.

Blood from a man in health.

Clot.	Dry Fibrin.
1000 grs.	4·42 grs.

EXP. LXXV.

Blood taken from a stout young man, ill with cough.

Clot.	Dry Fibrin.
1000 grs.	7·24 grs.

EXP. LXXVI.

Blood from a man having slight pleurisy. A slight fibrinous coat.

Clot.	Serum.	Dry Fibrin.
1000 grs.	932 grs.	7·05 grs.

EXP. LXXVII.

Blood from a man in health. The clot is soft and has thin edges. Specific gravity of this blood, 1·050.

Clot.	Serum.	Dry Fibrin.
1000 grs.	814 grs.	2·37 grs.

EXP. LXXVIII.

Blood from a man in health, and similar in appearance to the last. Its specific gravity, 1·052.

Clot.	Serum.	Dry Fibrin.
1000 grs.	853 grs.	4·43 grs.

EXP. LXXIX.

Blood from a man in health. Its specific gravity, 1·057.

Clot.	Dry Fibrin.
1000 grs.	3·29 grs.

EXP. LXXX.

Blood from a man in health. Its specific gravity, 1·0567.

Clot.	Dry Fibrin.
1000 grs.	3·84 grs.

EXP. LXXXI.

Blood from a man complaining of pain in the side. It has a slight fibrinous coat.

Clot.	Dry Fibrin.
1000 grs.	11·37 grs.

EXP. LXXXII.

Blood from a man ill with acute gout. It is free from fibrinous coat, but has a firm texture.

Clot.	Dry Fibrin.
1000 grs.	5·88 grs.

EXP. LXXXIII.

Blood from a robust youth in perfect health,

aged eighteen. It is soft in texture. Its specific gravity, 1·054. The serum high coloured.

Clot.	Serum.	Dry Fibrin.	Sp. gr. of the serum.
1000 grs.	543 grs.	4·27 grs.	1·030.

EXP. LXXXIV.

This youth, immediately after the bleeding, took running exercise for two miles. He was bled again directly afterwards, when his pulse was beating 96° and strong. The intention by this experiment was to ascertain whether increased circulation from exercise would give rise to the appearance of the fibrinous coat. The clot was soft like the first, and had the appearance of perfectly healthy blood. The serum was pale.

Clot.	Serum.	Dry Fibrin.	Sp. gr. of the serum.
1000 grs.	721 grs.	3·86 grs.	1·035.

EXP. LXXXV.

Blood from a robust young man in perfect health; soft in texture, its specific gravity, 1·050.

Clot.	Serum.	Dry Fibrin.	Sp. gr. of the serum.
1000 grs.	597 grs.	2·37 grs.	1·033.

EXP. LXXXVI.

Blood taken from a man when in the hot fit of an intermittent. It was received in five different cups. The first-drawn had a considerable fibrinous coat; the fourth a slight one; the last was free from such appearance. The first came from the arm in a full stream; the fourth in a slower stream; and the last very slowly as the patient was becoming faint; from which circumstance it became quickly coagulated. My object was to examine the real proportions of fibrin with relation to the appearances of the blood.

	Clot.	Dry Fibrin.
First cup....	1000 grs.....	5·77 grs.
Fourth.....	1000 grs	6·99 grs.
Last.....	1000 grs..... ..	7·95 grs.

EXP. LXXXVII.

Blood taken from a man ill with fever. It was received in two cups; in the first in a full stream, so that four ounces were drawn in one

minute; and the second portion was drawn purposely very slowly. The first coagulated slowly, and had a fibrinous coat, the last very quickly, and was free from such appearance.

	Clot.	Serum.	Dry Fibrin.
First cup	1000 grs	775 grs	4·68 grs.
Second..	1000 grs	770 grs	5·14 grs.

These results serve to show that we ought not to estimate the character of the blood merely from its appearance, without taking other circumstances into consideration, and especially the kind of stream in which the respective portion flows from the orifice.

It often happens that in a case of active inflammation, at the same bleeding, different cups of blood shall exhibit a remarkable difference of appearance; one having a thick fibrinous coat, and another being almost or wholly free from it. As a general statement, I am led to say, that in a case only slightly inflammatory, if each portion flow in a good stream, it is the latter or last cup which will not have the buffy coat, or have it but slightly; but if in consequence of the fear

of the patient, or other causes, the blood flow slowly at first, and in a good stream afterwards, the first-drawn portion will have less appearance of this coat than the last, on account of its quicker coagulation.

When the inflammatory disease is very severe, and the different cups of blood vary in appearance, the last-drawn almost always has the least of the fibrinous coat.

I was desirous to ascertain, whether the real proportion of fibrin corresponded with its apparently great excess in one portion of blood, compared with another, drawn at the same bleeding. In this example, the blood in the first basin had a remarkably thick buffy coat; and that in the second basin was free from it. The blood had flowed in an equal stream.

EXP. LXXXVIII.

In order to make this Experiment more instructive, the whole of the fibrinous coat was separated from the inferior coloured clot, and each was examined for the fibrin in the usual manner.

Fibrinous coat with a little adhering crassamentum.	Dry Fibrin.
1000 grs.	20·53 grs.
The inferior coloured clot.	Dry Fibrin.
1000 grs.	3·36 grs.
Whole clot of the second basin.	Dry Fibrin.
1000 grs.	5·76 grs.

In this example we see, in a striking manner, that when the fibrin is in great excess, it is only in a small proportion carried down with the red particles. The great disproportion also of fibrin in the two basins is a circumstance worthy of consideration.

The experiment was repeated; another case presenting itself in which the blood in the first basin had a thick buffy coat, and in the last was free from such appearance.

EXP. LXXXIX.

Fibrinous coat as before.	Dry Fibrin.
1000 grs.	16.50 grs.
The inferior coloured clot.	Dry Fibrin.
1000 grs.	4·16 grs.
Whole clot of the second basin.	Dry Fibrin.
1000 grs.	6·25 grs.

An opportunity occurred to me of investigating the comparative proportions of fibrin in venous and arterial blood.

EXP. XC.

A patient suffering from ophthalmia was bled in the arm and in the temporal artery, in immediate succession; and the blood from the vein and artery was each received in two basins. The first portion of blood from the arm came in a slower stream than the last, and coagulated soonest. It was free from the appearance of fibrin; the last had a considerable buffy coat. These circumstances took place exactly in the same manner in regard to the blood from the artery. The first portion came in a slower stream than the last, coagulated soon, and had no appearance of fibrin: the last had a strong buffy coat.

Clot of first portion of venous blood.	Dry Fibrin.
1000 grs.	10·09 grs.
Clot of second venous portion.	Dry Fibrin.
1000 grs.	10·29 grs.

Clot of first portion of arterial blood.	Dry Fibrin.
1000 grs.	12·16
Clot of second arterial portion.	**Dry Fibrin.**
1000 grs.	10.86

EXP. XCI.

I repeated, in the case of another patient, the examination of blood taken at the same time from the arm and from the temporal artery.

Clot of venous blood.	Proportion of serum.	Dry Fibrin.	Sp. gr. of the serum.
1000 gr.	1077 gr.	11·76 gr.	1·0265
Clot of arterial blood.	**Serum.**	**Dry Fibrin.**	**Sp. gr. of the serum.**
1000 gr.	806 gr.	10·33 gr.	1·027

Respecting the external character of blood taken away by cupping, I have to observe, that, except in one instance, I never saw a well-marked appearance of the buffy coat. The disease was in the brain, the patient in a state of extreme debility; the blood was fluid when the glass was emptied. It is to be considered that the blood under the cupping-glass is similarly circumstanced as when placed under the air-pump. It quickly parts with its carbonic acid, and from this cause coagulates very soon, but also the coagulation is

hastened by means of the high temperature of the cupping-glass, which is put into hot water before its application.

EXP. XCII.

A patient was bled in the arm, and cupped in the neck, at the same time. Each portion of blood was of healthy appearance, but more than usually firm in texture.

Clot of blood drawn from the arm.	Serum.	Dry Fibrin.	Sp. gr. of the serum.
1000 gr.	666 gr.	7·81 gr.	1·031

Clot of blood taken by cupping.	Serum.	Dry Fibrin.	Sp. gr. of the serum.
1000 gr.	632 gr.	6·93 gr.	1·030

EXP. XCIII.

Blood taken from the neck ; the patient complaining of sensations of fulness in the head, but otherwise in health.

Clot.	Serum.	Dry Fibrin.
1000 grs.	1067 grs.	3·50 grs.

EXP. XCIV.

Blood taken from the neck of a robust young man who had been for several days in a state of intoxication.

Clot.	Serum.	Dry Fibrin.
1000 grs.	375 grs.	5·72 grs.

EXP. XCV.

Blood taken from the neck. The patient a slight young man labouring under delirium, much debilitated.

Clot.	Serum.	Dry Fibrin.
1000 grs.	954 grs.	3·51 grs.

EXP. XCVI.

Blood taken from the chest of a young man ill with hæmoptysis. Pthisis evidently established.

Clot.	Serum.	Dry Fibrin.
1000 grs.	714 grs.	11·44 grs.

General Conclusions and Medical Observations.

As the preceding recital of experiments may not appear sufficiently instructive, I shall, in taking a general review of my subject, endeavour to offer some further explanation, and to advance some theoretical reasoning, which may at least

serve the purpose of exciting more inquiry and investigation.

The action of heat in promoting, and of cold in retarding, the coagulation of the blood, was pointed out by Mr. Hewson, although he does not seem to have been fully aware of the remarkable power of heat in accelerating coagulation. He considered the air to have a great influence; and, indeed, that coagulation was chiefly owing to this cause. He found that rest merely was not sufficient to account for it, by his experiment of confining the blood of the dog in the jugular vein by ligature, for a much longer time than is sufficient for coagulation when blood is exposed to the air. The hastened coagulation of the blood in vacuo, notwithstanding that the temperature becomes considerably reduced, has not, I believe, ever been explained. The succeeding experiments, shewing that the mere absence of atmospherical air was not the cause of the hastened coagulation, led me to the examination respecting the carbonic acid gas; and I think myself fully authorised in concluding that the period

of time in which the blood coagulates, depends, in a great measure, on the quick or slow extrication of the carbonic acid gas. Its evolution takes place most freely as the blood begins to concrete, and ceases when coagulation is completed. It is evidently an essential circumstance in the process of coagulation, as the same causes which retain the carbonic acid in the blood, delay coagulation.

Sir Everard Home, in some ingenious papers in the Philosophical Transactions, has described the phenomenon of the extrication of carbonic acid gas from the blood during coagulation, but was not led to the opinion which I have adopted, of its being so essential a step in the process, the time of coagulation simply considered. Sir Everard had another object in view, and although it may be felt as a digression from my immediate subject, I cannot refrain from adverting to the novel and ingenious theory set forth in these papers, respecting the effect of the extrication of carbonic acid from the blood. The author states that during the coagulation of the blood, chan-

nels are formed in the coagulum by means of the

evolution of the carbonic acid gas contained in the blood; and that in the living body new organization is effected by means of the blood-vessels transmitting red blood into these channels, and thus establishing vascularity. The formation of such channels was made apparent to Sir Everard's satisfaction by the aid of Mr. Bauer's very powerful microscope; but the foundation of the whole opinion seems to be derived from the following statement respecting the carbonic acid gas.

* "The first object of our enquiry was to know whether any gas is to be found in the blood while circulating in the vessels, and under what circumstances it is separated from it. That the blood, while circulating in the arteries and veins, holds a considerable quantity of gas in solution, is proved by the following experiments, made at my request by Professor Brande. Blood was drawn from a vein in the arm, and, while yet warm, was placed under the receiver of an air

* Phil. Trans. 1818, p. 181.

pump; during the exhaustion of the receiver there was a considerable escape of gas from the blood, so that it had the appearance of effervescing, and soon depressed the quicksilver in the gauge of the pump. He afterwards ascertained that this gas is carbonic acid gas, is met with in the same proportions in arterial and venal blood, and two cubic inches were extricated from every ounce of blood."

With the highest respect for the authority thus quoted, I could not avoid being sceptical as to this large proportion of carbonic acid, and with the assistance of Mr. Garden, I made a series of experiments upon this preliminary point.

We first employed a glass matrass furnished with two openings. To the one opening was adapted a glass tube bent at right angles, and connected with a bottle of barytic water. The other aperture was provided with a glass tube of a conical form; so that the widest end could be placed exactly over the orifice of the vein. By this arrangement, the blood was made to enter the matrass with comparatively little contact

with the atmosphere. Heat was carefully applied to the bottom of the retort so as to expel the carbonic acid; but we found that by its agency coagulation took place too readily, and less proportion of gas was procured than by the following method, and which, I understood, was similar to that adopted by Mr. Brande.

Six ounces of blood were received in a pint bottle, and this was promptly placed under the receiver of an air pump. The receiver was then slowly exhausted, and the action of the pump kept up so long as any gas continued to be evolved. The extremity of the tube (made of flexible metal) was perforated with holes, so that the gas should escape more gradually, and have a better opportunity of combining with the barytic water. The pump was worked carefully but effectually, till gas ceased to come out. The bottle of barytic water was not removed from the receiver, until the carbonate of barytes which had been formed during the process of exhaustion, had completely subsided; and this care being observed, none of the precipitate was separated

in the regurgitation of the liquid into the bottle containing the blood, on the admission of air.

A man in perfect health was bled two hours after a hearty dinner. Six ounces of his blood furnished forty-eight hundredths of a cubic inch of carbonic acid gas.

Six ounces taken from a man bled on account of a cough, before his dinner, furnished forty-six hundredths of a cubic inch.

Six ounces taken from a young woman in good health, before dinner, furnished only thirty-one hundredths of a cubic inch.

The two men lived heartily on animal food, and drank porter daily. The one who was the subject of the second experiment, had, on account of his cough, lessened his usual quantity. The young woman ate animal food, but drank water.

In another experiment, blood was taken from a young man in health, before dinner, and again two hours after; and it is well worthy of observation, that the blood drawn after dinner, yielded full double the proportion of that obtained from the

blood taken away before the meal. But the largest quantity which, in any instance, we have been able to obtain, has been rather less than half a cubic inch from six ounces of blood; a result remarkably in opposition to that quoted by Sir Everard.

He relates an experiment of injecting the coagulum of blood which had been taken from the arm forty-eight hours previously. Common red size injection, in a fluid state, was used; it being poured into a cavity made in the coagulum by cutting away a portion. The containing vessel was put under the air pump. "After the exhaustion had become nearly complete, the glass vessel and its contents were removed, and with a view to fix and harden the coagulum, the glass vessel was placed in boiling water, which was renewed at short intervals; carefully preventing the water from coming in contact with the blood. This process melted the injection that had not passed into the coagulum, and allowed of its being poured off. The coagulum even now was by no means very firm, but capable of supporting it-

self; it was turned out upon a flat piece of glass. The coagulum was afterwards divided into thin slices; and its internal substance was found minutely injected. The author concludes with observing, that, "as the injection could only fill the spaces from which the carbonic acid gas was extracted, it cannot be doubted that the channels were formed by the gas."

As a reader of the ingenious experiments in question, I desire, in the spirit of inquiry, rather to doubt than too readily believe, results so novel, pleasing, and curious. It is not at present convenient to me to make a series of exactly similar experiments; but arguing from those which I have instituted, I shall exercise all the freedom of philosophical discussion, in proposing some questions as to the conclusion drawn by Sir Everard Home, that blood taken from the body, as it becomes coagulated, has channels formed through it by the extrication of carbonic acid gas; and also, "not only that the same change takes place when blood is extravasated in living animal bodies, but that these channels have a communi-

cation opened between them and the neighbouring arteries, and that the fluid blood circulates through the channels in the coagulum." It would appear to me as a probable circumstance, that the quantity of carbonic acid gas contained in the blood, is too small for the purpose stated; and also, that it would not be evolved in straight or curved lines, but in globules, the very objection indeed mentioned by the author; but, I think, not satisfactorily answered. A net work of globules viewed in the field of a very powerful microscope, may be expected to present appearances which are very fallacious. According to the description at page 182, I can readily suppose the representation of a globular net-work; but it is not so easy to believe in the existence of channels fitted to receive circulating blood. In the experiment with the coagulum under the air pump, just now quoted, I cannot satisfy myself that the force exerted on the coagulum by the process of exhaustion, is analogous to the gentle natural action of the evolution of carbonic acid gas in the ordinary coagulation of the blood; and which

gas appears to the naked eye, on the surface of the blood, as a honey-comb structure. I have witnessed repeatedly that the extrication of the carbonic acid gas, and the atmospherical air contained in blood, coagulating or coagulated, effected under the exhausted receiver of the air pump, so breaks up the continuity of the mass, as that it may be well supposed to produce fissures or channels capable of admitting a size injection; but I conceive this to be completely artificial, and not an imitation of what takes place in the natural coagulation of the blood. I suppose it is not meant that any channels are formed before the blood becomes solid; and afterwards, such portion of carbonic acid as may not have escaped during the process of coagulation, will surely be imprisoned in the coagulum.

In inflammation, or in that action which belongs to reparation, or regeneration of parts, a layer of fibrin is deposited. If the effect of inflammation, it appears to be effused from the extreme vessels; but, if the entire blood be poured from wounded vessels, I conclude that

the red particles and serum, more or less gradually become absorbed, leaving the fibrin. The ordinary opinion that the extreme capillary vessels shoot into the delicate layer of fibrin, and quickly organise it, building it up as it were into the particular fabric required, seems to me, to constitute a theory sufficiently complete. But, in conclusion, I beg to state, that in the observations which I have offered, I wish to be considered as expressing doubts rather than contradictions, of the justness of the conclusions, which Sir Everard Home has drawn from his truly ingenious experiments.

It may be stated as a general rule, that blood coagulates in the shortest time, accordingly as it is of high specific gravity. Such is the blood of a strong and healthy person, as abounding most in red particles, which constitute the heaviest part of the blood. Mr. Hunter, in speaking of the red globules, remarks that "their use would seem to be connected with strength, for the stronger the animal the more it has of the red globules." The fibrin also belonging to such healthy blood is

more dense than that of blood in disease, and hence the real explanation of a quick coagulation.

The very marked difference of time in which the blood coagulates, accordingly *as the stream from the orifice is fast or slow*, appears to me to warrant the conclusion which I have stated, that the commencing part of the process of coagulation, namely, the escape of carbonic acid, does take place more readily when the blood flows very slowly.

It appears that *rest*, merely, does not assist the coagulation of the blood. We see in how remarkable a manner cold delays coagulation of the blood in the basin ; and how slowly it takes place when confined in a vessel of the living animal. Comparatively, also, as shown in Experiment xxxvi, the blood which was left perfectly at rest, coagulated more slowly than that portion which was gently stirred.

The kind of vessel in which blood is received in venesection, deserves attention. If it be metallic, the coagulation of the blood is slow from the material being a free conductor of caloric ;

and the blood separating more gradually into its component parts in the order of their specific gravity, will more probably exhibit the buffy coat than when a porcelain cup is used.

The proportion of fibrin and its quality, as having an influence on the coagulation of the mass, is a point of material consideration. We must keep in view, that the solidification of the fibrin is the essential cause of coagulation. The process is promoted by heat "for two reasons." The carbonic acid is driven off sooner, and the solidification of the fibrin is evidently assisted. The temperature required to render the fibrin almost immediately solid, varies according to the density of the fibrin. Thus in an example of moderately sizy blood, collecting the fibrin while liquid and mixed with the serum, I found that it required a heat only of 120° to render it solid, and the serum 140°. The density of this blood was but little below that of the most healthy blood, for the patient was bled for the first time; but in another example in which the fibrin very greatly predominated in quantity, the

same degree of effect required a heat of 130° for the fibrin, and of 160° for the serum. This blood was of very low specific gravity. The patient had been bled several times, and was much reduced.

In attempting to explain the coagulation of the blood, I should be induced to say that it depends on a new condition of the fibrin, which can only exist fluid in a state of intimate mixture with red particles, serum, and carbonic acid gas. Except in circulation or in living vessels, the fibrin will not maintain an union with the serum, although it does continue blended with the red particles, either when extravasated in the living body, or when taken from the vessels. It is therefore the property of the fibrin to become solid when separated from the several principles just stated, constituting in union the mass which we call blood. It will perhaps be asked, how does it happen that such blood as contains the largest quantity of fibrin coagulates the most slowly, when coagulation depends upon the fibrin? I do conceive, that the larger the

proportion of fibrin is in healthy blood, as its proper constituent, the more rapid will be its coagulation.

I wish to observe also, that such blood which I advert to as the most healthy, contains the largest proportion of red particles, as may be deduced from its high specific gravity. Indeed, in blood of perfect health, its constituent parts may be supposed to exist in a constant relative proportion to each other, and that every considerable difference from such proportions ought to be viewed as a deviation from the best standard.

To the circumstance chiefly of a large proportion of fibrin as belonging to health, I am disposed to attribute the immediate coagulation of the blood of the sheep; in which I have found the proportion of fibrin to 1000 grains of crassamentum 7·29 grains. The mean proportion in eleven examples of human healthy blood, was 3·72 grains. In answer to the question just now proposed, I contend that when the fibrin is in great excess, it is less dense than in healthy blood, and takes the solid form more slowly. As ex-

plained in my experiments, the excess is so abundant in very sizy blood, that the larger proportion remains in the upper part of the basin in admixture with the serum ; and hence its slower coagulation. Also that portion which falls down with the red particles, is less dense than natural, and coagulates rather slowly. When the fibrin is in great excess, it would seem that the red particles cannot blend themselves with the whole quantity as in ordinary coagulation.

Mr. Hewson was correct in stating, that the coagulable lymph of sizy blood has more tenuity than that of healthy blood. This position was denied by Mr. Hey, who considered that the appearance was owing to the admixture of serum with the coagulable lymph, which does happen as I have shewn ; but the question of specific gravity must be determined by nicer observation than appears to me to have been employed by either of these authors.

I find that sizy blood abounds in serum more than healthy blood ; but the large quantity which appears, is partly owing to its being forced out

from the clot by the strong contraction of the fibrin. In such blood, you find the coagulum distinguished by inverted edges, swimming in a large quantity of serum. It thus floats because the proportion of fibrin which is of the least specific gravity, exceeds the usual proportion which it bears to the red particles.

Dr. John Davy, in his observations on the buffy coat of the blood*, appears to consider, that coagulation is not always retarded in sizy blood, remarking in substance, as I have pointed out, that to the subsiding part of the mass we must look for the commencement of coagulation. In regard to the formation of the buffy coat, he adverts to the tenuity of the fibrin, but does not seem aware of the great excess of it which exists in very sizy blood.

Sir Everard Home† also adopts Mr. Hewson's explanation of the buffy coat of the blood. He remarks, that "the buffy appearance occurs when the lymph is so unusually slow in coagulating,

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* Phil. Trans. 1822, Part II. † Ibid. 1820.

that the red globules which are so much larger and heavier, sink before that process has taken place."

The fact most difficult of explanation, is the variable proportion of fibrin in different cups of blood drawn at the same bleeding; sometimes a difference of more than half between the contents of two cups. For example, in one instance of sizy blood, I found the proportion in the first cup for 1000 grains of clot, twelve grains; and in the last cup, not quite six grains. The highest proportion that I have found in healthy blood, has been for 1000 grains of clot, 4·43 of fibrin. It is in inflammation of the fibrous textures of the body, that the greatest quantity of fibrin is found in the blood; for example, in inflammation of the heart, when blood is drawn, we meet with the strongest example of the buffy coat; and next, probably, in pleurisy, and acute rheumatism. I am led, therefore, to the idea, that, under such circumstances, the fibrin, instead of being distributed to the fibrous textures as usual in health, remains in excess in the blood. The

textures in question would be injured by receiving their usual supply of fibrin, being now in a state of disease; and as regards the variation in the quantity of fibrin in different portions of blood drawn at the same time, it does not appear to me a difficult supposition that in the very short space of time occupied in venesection, the state of circulation should change, so that the capillary arteries at once make a different distribution of the fibrin; resuming in great measure their ordinary economy, and conveying it to the fibrous textures instead of returning it in unnatural excess to the venous circulation. The fact itself of the different proportions of fibrin in different cups of blood filled at the same venesection, is clearly proved. I know not any more probable theory to offer in explanation.

I shall resume the consideration of the fibrinous coat, in the medical division of my subject.

The Blood as being Arterial or Venous.—In the comparison between the arterial and the venous blood, as to the time of coagulation, I conclude from my experiments, that the only cir-

cumstance which has an influence on the coagulation of the two kinds of blood taken from the same subject, is, the slow or fast stream, which causes the carbonic acid to pass off more or less freely. In blood taken from different individuals, or different animals, the result would of course also be controuled by the difference of density in the respective portions of blood.

The influence of *the vital principle* is an interesting question, as it leads us to the examination of Mr. Hunter's opinion on this part of the subject. After considering the influence of cold, air, and rest, and concluding that none of these causes alone, nor all of them combined, induce the blood to coagulate, he observes, "My opinion is, that it coagulates from an impression, that is, its fluidity under such circumstances being improper, or no longer necessary, it coagulates to answer now the necessary purpose of solidity. This power seems to be influenced in a way, in some degree similar to muscular action, though probably not entirely of that kind; for I have reason to believe that the blood has the

power of action within itself, according to the stimulus of necessity, which necessity arises out of its situation." He further adds, "Coagulation, I conceive, to be an operation of life; and I imagine it to proceed exactly upon the same principle as the union by the first intention. It is particle uniting with particle by the attraction of cohesion, which, in the blood, forms a solid." Mr. Hunter distinctly considers that the fibrin possesses a living principle. He every where speaks of its coagulation as a living action. Thus, "the life of the seed stimulated to action by the moisture, is the immediate cause of its growth; and it continues to grow because its action is always exerted. All the water in the world would not make a dead seed grow. The same mode of distinction is applicable to the coagulation of the lymph." In the same manner that the hatched egg contains "the power of self preservation," or, in other words, the simple principle of life, so did Mr. Hunter believe that the blood "has life within itself."

I shall confine myself to the following additional quotation.

"The prevention of coagulation may be effected by electricity."

"Animals killed by lightning, and also by electricity, have not their muscles contracted; this arises from death being instantaneously produced in the muscles, which therefore cannot be affected by any stimulus, nor consequently by the stimulus of death. In such cases, the blood does not coagulate. Animals who are run very hard, and killed in such a state, or, what produces still a greater effect, are run to death, have neither their muscles contracted, nor their blood coagulated; and in both respects, the effect is in proportion to the cause."

"Blows on the stomach kill immediately, and the muscles do not contract, nor does the blood coagulate. Such deaths as prevent the contraction of the muscles, or the coagulation of the blood, are, I believe, always sudden. Death from sudden gusts of passion, is of this kind; and in all these cases, the body soon putrifies after death."

"In a natural evacuation of blood, viz. menstruation, it is neither similar to blood taken from a vein of the same person, nor to that which is extravasated by an accident in any other part of the body; but is a species of blood changed, separated, or thrown off from the common mass, by an action of the vessels of the uterus, similar to that of secretion; by which action, the blood loses the principle of coagulation, and I suppose life."

Although I believe that these favourite opinions of Mr. Hunter no longer obtain general credit*, yet as they still have their advocates, it seems incumbent on me to offer some comments founded on my own experiments, whence I hope to demonstrate that the changes which the blood undergoes, when drawn from its vessels, are entirely of a chemical nature.

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* My feeling of admiration of the genius of this great man, and of the general merit of his most able and important Treatise on the Blood, will, I trust, be reconciled with my criticism of his hypothesis of the vitality of the blood.

When the blood is insulated or removed from the vessels of the living animal, its tendency is to separate into its component parts; and the first essential step in the process of coagulation, is the giving off the carbonic acid. The fibrin solidifies as its characteristic property, when no longer in intimate union with the other parts of the blood in living vessels. But by what simple causes is this effect prevented? by the addition of common salt, and still more completely by that of the liquor potassæ. On the other hand, the prussic acid, and the extract of belladonna, the one when fully applied, so destructive to life, and the other so powerful in relaxing the muscular fibre by its action on the nerves, do not prevent firm coagulation.

The living principle of the seed, and of the hatched egg, cannot be offered as proper illustrations of the condition of the coagulation of the blood, which is destitute of all reproductive power, and is merely the material on which, under certain circumstances, the living vessels act.

With regard to the coagulation of the blood out of the body, I find that it is rather promoted by electricity, as shown in Exp. liii and liv. I do not understand whether Mr. Hunter intended to say that electricity would prevent coagulation of the blood drawn from the vessels.

I have not had the opportunity of examining the state of the blood in animals killed by lightning or hunted to death; but in the various experiments which I made of destroying animals by electricity, I have invariably found the blood coagulated as usual. When sudden death is produced, we know that the putrefactive process takes place very quickly, and therefore the blood soon becomes decomposed after such kind of death; especially in warm weather*. A dog

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* A gentleman was found dead in his bed, and as but little time could have elapsed since the fatal moment, a vein was opened in the arm. The weather was warm. Putrefaction commenced so rapidly, that the profuse flow of the decomposed blood from the orifice, occasioned great distress and inconvenience.

was killed instantaneously by means of prussic acid, and the fibrin was found separated in the left ventricle and in the aorta; so that, at first sight, the blood appeared fluid. In other instances, I have seen the flow of serum mixed with red particles giving the appearance of blood not coagulated, when, on closer examination, I have found the fibrin detached.

Of the secretion in menstruation, I have merely to observe, that, as it does not contain fibrin, it has not the power of coagulating. Mr. Brande observes, when speaking of this secretion, "It had the properties of a very concentrated solution of the colouring matter of the blood in a diluted serum.*"

The influence of the living principle in parts having contact with blood, is very manifest. If a leech survive its suction, and be examined several weeks after, the blood will be found grumous, but free from signs of decomposition, and it coagulates on being exposed. In one instance,

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* Phil. Trans, 1812. p. 113.

upon examining a leech about three months after it had been used, I found that the red particles were almost wholly removed; but there was left a string of fibrin. If the leech die, the blood quickly becomes putrid.

Mr. Thackrah, in an "Inquiry into the Nature and Properties of the Blood," attempted to prove that the coagulation of the blood is considerably delayed, if received "in a vessel recently insulated, or recently removed from a living animal;" but the results of my experiments, p. 49, &c. are in opposition to this opinion. He concludes with stating that "the vital or nervous influence is the source of the blood's fluidity, and its loss the cause of coagulation."

I will here mention again the experiments which were made on the blood of the horse, to illustrate the influence of the living vessel not yet removed from the animal.

An inch and a half in length of the jugular vein was included between two ligatures, and at the end of an hour was emptied of its contained blood, which was quite fluid, but which coagu-

lated in five minutes after being received in a cup.

The experiment was repeated, and, at the expiration of one hour and three quarters, the blood was fluid, but coagulated in the cup in which it was received in five minutes.

In these examples, we can hardly consider the blood as out of the circulation ; presuming that the time had not been sufficient to destroy the life of the vessel included between the ligatures.

Observation was made on the state of the blood in the vein immediately dissected away and removed from the animal, after the ligatures were passed. At the interval of forty minutes, the blood was thickened, but not actually coagulated. I should add, that the vessel was exposed to a cold atmosphere ; and, under these circumstances, the carbonic acid would pass off so gradually, that coagulation would be a slow process.

It is made evident, that the condition of the blood is materially influenced by its being merely in contact with living parts. The fact already men-

tioned of the blood not becoming putrid in the body of the leech surviving suction, proves this. Out of its proper vessels, the blood coagulates in the living body ; as in the cellular membrane ; or in an aneurismal sac ; and also in the vessels themselves when they have lost their life. Mr. Hunter observes, " I have seen a mortification come on the foot and leg, and when it had advanced only to a certain degree, the person died. On examining the parts above the mortified part, I found the crural and iliac arteries filled completely with strong coagulated blood. We may thence infer, that the tendency to mortification in the vessels produced the disposition in the blood."

We now arrive at the conclusion, that in all circumstances when the blood is in vessels forming part of the living body, even in animals which are torpid during the winter season, it is fluid ; but coagulates, or maintains a disposition to coagulate, when the living power of the vessels has ceased, unless the putrefactive process has taken place more or less.

The influence of *electricity*, and still more of

galvanism as being the stronger agent, in raising the temperature of the blood, distinctly shewn in Experiment liii to Experiment lvii, is probably to be ascribed to the direct power of imparting heat which these peculiar stimuli possess*. The electric fluid does not visibly alter the natural arrangement of the blood in coagulating; but the galvanic action causes instantaneous decomposition.

The effect of *oxygen gas* in raising the temperature of the blood, is rendered sufficiently manifest in Experiment lviii and lix; and the result very naturally leads us to the theory of animal heat as depending on the combination of oxygen with the blood in the lungs. If the blood out of the body receive sensible heat from its combination with oxygen, may we not consider that the same effect takes place in the lungs? Dr. John Davy, in his valuable paper on animal heat†,

* In Experiment lii, with electricity, I found that the thermometer which was inserted in the rectum of the dog, rose each time half a degree, immediately on the application of the shock; an effect to be referred to the influence of electricity on the nervous system.

† Phil. Trans. 1814, Part. II.

states, as the result of his experiments, that the temperature of the substance of the lung in a lamb, examined about a quarter of an hour after death, was 106·5, of the left ventricle of the heart 107°, and of the right ventricle 106°. He remarks, that his experiments are "in direct opposition to Dr. Crawford's hypothesis, the essence of which is, that the capacity of arterial blood for heat is greater than that of venous; that there is no difference of temperature between the two ventricles of the heart; and in fact that the heat of all parts is nearly the same."

He adds, "they are more agreeable to, and indeed they even support, the hypothesis of Dr. Black, that animal heat is produced in the lungs, and distributed over the whole system by means of the arterial blood.

"Neither are they inconsistent with that hypothesis, which considers the production of animal heat, as dependant on the energy of the nervous system, and arising from all the vital actions constantly occurring."

In the latter observations, Dr. Davy alludes

to the important experiments of Mr. Brodie. That the blood under the influence of oxygen should coagulate the soonest, in Experiment lviii, is a result naturally to be expected, as the comparative difference of temperature was so remarkable.

In Experiment lx with *hydrogen gas*, the difference of temperature was three degrees in favour of the blood exposed to the air of the apartment; and the quicker coagulation of the blood which had been placed under the exhausted receiver, before being subjected to the influence of the hydrogen gas, must, I conceive, be attributed to the active removal of the carbonic acid gas from the blood.

In Experiment lxxi with *carbonic acid gas*, the difference of temperature was no less than nine degrees in favour of the blood exposed to the air of the apartment, and in this case the blood under the air pump charged with the introduced carbonic acid, coagulated more slowly than the other portion.

In Experiments lxii and lxiii, the effect of

azote was not so perfectly marked, except as regarded the dark colour and the loose texture of the blood.

The effect of different *chemical agents* on the fresh-drawn blood, is not uninteresting. It may be remarked, that the sulphuric acid itself, diluted as the Pharmacopœia directs, blackens the blood; but its combinations with alkalies produce a florid red colour. The muriatic acid, free and combined, very much darkens the colour of the blood. The nitric acid, in a free state, caused the blood to remain of its usual dark colour; but, combined with alkali, produced a vivid red appearance. It appears, that saturated solutions of several of the salts have the property of suspending the coagulation of the blood; but when the necessary quantity of water is added, the coagulating process takes place more or less perfectly. Hence we see, that no positive change is produced in the chemical constitution of the blood in the first instance, so as to prevent the solidification of the fibrin.

The trials with the other salts were made in

reference to their styptic quality, as depending on their chemical union with the fibrin, so as to form a coagulum. I shall afterwards notice this part of the subject.

In the account of my experiments to determine whether free caloric is evolved during the coagulation of the blood, I have offered all the comments which I think necessary; and I proceed now to those *medical considerations* which arise out of the experimental part of my inquiry.

In the examination of the blood in venesection, the practitioner must direct his attention to several circumstances; amongst which, the following may be mentioned as the principal: the kind of orifice; the stream in which the blood flows; the state of the pulse during the bleeding; the apparent density, the colour, and other appearances of the blood, on its being first received in the basin; the time in which it coagulates; the external appearance of the coagulated blood; the texture of the coagulum; the comparative conditions of the several portions of blood in dif-

ferent cups; the appearance and quality of the serum.

As appears from Experiments xxxiv and xxxv, the mode of coagulation of the blood is materially influenced by the kind of stream, which of course depends chiefly on the orifice made in venesection. When it flows slowly, the coagulation takes place so soon, that the fibrin unites itself with the red particles very uniformly, and therefore does not present any appearance of the buffy coat, unless the blood be remarkably sizy. I cannot, however, refrain from taking notice of a more important point as regards the stream of blood, namely, its remedial influence on disease. In venesection for the relief of inflammation, our object is not simply to diminish the quantity of circulating blood; but to diminish the action of the heart; and it may be affirmed, that twelve ounces of blood drawn so quickly as to produce an immediate and decided effect on the pulse, will prove much more useful than a considerably larger quantity stolen away so slowly that the heart accommodates itself more easily to the loss.

In a case of plethora, or apoplectic congestion, the same reasoning does not so exactly apply, because the absolute quantity of blood to be drawn is more to be considered. Invariably, however, a good orifice is important; and hence, it is of great consequence that the operation of venesection should be well performed.

In the case, and not an unfrequent one, in which the patient still labours under inflammation, but is now become much reduced by treatment, we find it desirable to diminish the action of the heart, at the smallest expense of the constitutional strength. Here then, let the orifice be free, and let the patient sit up, so that fainting may be induced by a small loss of blood.

The late lamented Dr. Pemberton was well aware of the importance of the manner in which the operation of bleeding is performed; as the following observations which I quote from his Treatise on the diseases of the abdominal viscera, will shew.

" As I consider this matter of great consequence, I shall endeavour to point out a method,

by which a plan of a more defined nature than that hitherto adopted, may be given for drawing blood in inflammatory diseases."

"At present we are contented to order, that the blood should be taken from a large or from a small orifice, than which nothing surely can be more vague or undefined. The plan, which I propose, refers to the *length of time* in taking the blood, which may be measured, and not to the size of the orifice, which can not.

"I find from numerous experiments, made at my desire for this purpose by different surgeons, that when the orifice is such as to permit eight ounces of blood to flow in three minutes, that then, a patient under acute inflammation will receive every benefit which is expected from the remedy. If it flows in a longer time he will receive less benefit, and under certain circumstances no benefit at all, or even an absolute injury."

"I can suppose a case of peripneumony, where a patient shall have just general strength enough to carry on respiration by the assistance of the voluntary muscles, and that eight ounces

of blood shall be taken from a very small orifice, by which the change will be so gradual, in consequence of the time required for the blood to flow, that no alteration whatever will be made in the disease, but yet the general strength shall be so diminished, that death will ensue: on the other hand, had the same quantity of blood been taken by a large orifice, that *then* the disease would have felt the remedy, and respiration would have gone on with less exertion of the remaining general strength, in consequence of the lungs being relieved by this sudden depletion."

In every inflammation the rule is important; but in peritoneal inflammation, I hold it to be very highly instructive, that the physician should have his finger on the pulse of the patient during the whole time of the operation of venesection; and found his judgment of the quantity of blood which it may be expedient to draw away, much more by the change of action in the pulse, than by the mere quantity of blood abstracted. As a general rule, I conceive it may be stated, that the flow of blood should not be stopped until the

hardness and frequency of the pulse are most distinctly reduced. In cases of oppression of the circulation, as in inflammation and obstruction of the lungs particularly, the pulse rises and becomes fuller after the abstraction of several ounces of blood; but hardness and frequency of the pulse, are points of separate consideration from its mere volume. I must now take leave of this discussion.

The colour of the blood as it flows from the vein, is influenced by different causes. If the ligature have been kept on the arm for some time, the blood which first flows will appear very dark. In obstructed respiration, particularly in a fit of asthma, the blood presents an almost black appearance in consequence of its difficult transmission through the lungs; and consequently imperfect oxygenation. In quick respiration, as in pthisis pulmonalis, the blood drawn usually appears very florid. I believe that in all inflammations, the blood drawn from the veins is more florid than natural, unless the circulation is obstructed, so that the blood passes with difficulty from the arteries to the veins.

We may soon judge of the density of the blood on its being received into the basin. The appearance to the eye gives some information, but we are chiefly instructed by the time in which it coagulates. Under the head of specific gravity, I have given examples to shew, that the heaviest blood coagulates the most quickly. It is well known that blood which will furnish the buffy coat in a great degree, in the course of a few minutes presents a watery appearance, and the finger, being applied to the surface, is not stained with blood. We may also notice before the blood becomes quite coagulated, whether it has a frothy appearance, as indicative of a large proportion of fixed air, taking into the account, that if it has passed in a full projecting stream, more atmospherical air will be entangled, than if it flowed down the arm.

I believe that these are the chief circumstances to be noticed in the immediate appearance of the blood, when passing, and just received into the basin.

The blood should be received in different

cups; and the breakfast cup is the most suitable size. They should be put by in order, all in the same situation as to temperature, and not disturbed, so that we may be enabled to draw some useful inference of the effect produced on the circulation during the operation of venesection, by the relative appearances of the different portions of blood. In deciding upon the mere presence or absence of the fibrinous coat, we must particularly refer to the quick or slow stream in which the blood has flowed. In active inflammation of the fibrous textures, the formation of the buffy coat cannot be prevented, because the fibrin in the blood is in such excess; but when inflammatory action of the circulation does not much prevail, we shall learn more of the nature of the blood, from examining its texture, than from merely viewing the surface of the coagulum. I consider that the texture of the coagulum and its degree of contraction indicate more of the actual state of the heart and arteries, than the mere presence or absence of the fibrinous coat. In the examination of blood taken away by cupping,

we have to form our judgment chiefly from the texture of the coagulum, as to the eye it almost constantly presents an uniform appearance. In general terms, I may state, that a firm texture of the blood points out a strong action of the blood vessels, so as to give a presumptive sign that the bleeding has been proper; and vice versa, if the coagulum be remarkably loose in texture, we should particularly question the propriety of repeating the operation. I may further remark, that when the clot possesses uniform firmness, and has its edges turned inwards, we may conclude that the blood vessels act more strongly than when it is soft in its texture throughout, and having a thin and flabby edge. The extreme toughness of the buffy coat itself, and this contrasted with the soft texture of the inferior part of the coagulum, is matter of familiar observation.

The quantity of *serum* which appears in the basin, depends chiefly on the more or less firm contraction of the clot. It most abounds when

the blood presents the buffy coat, because the strong contraction of such a clot forces out the serum.

Respecting the external appearance and qualities of the serum, I am not aware that very material conclusions are to be drawn. Its density cannot be judged of by the colour.

Experiment lxxxiv shews, that the palest serum had the highest specific gravity. When the bile has found its way into the circulation, it tinges the serum strongly. If a person be bled shortly after the dinner meal, the serum appears milky. I believe that this appearance is owing to the recent introduction of chyle into the circulation; and it seems to me to warrant the idea, that several rounds of the circulation take place, before sanguification, or the complete conversion of chyle into blood, is effected.

The ordinary notice which is taken of the appearance of the *coagulated blood*, is for the most part in reference to the question, whether or not it indicate the inflammatory diathesis. Has it

the buffy coat? or, in more common language, and certainly very incorrect language, is it inflamed, or not?

In the second volume of the Medical Transactions, the late Dr. Heberden proposes as a query, "Is the sizy covering which is often seen upon blood, of any use in directing the method of cure?" He concludes his paper with the following remarkable passage:

"The more we know of the human body, the more reason we find to believe that the seat of diseases is not to be sought for in the blood, to the sensible qualities of which they seem to have very little relation; and though it be supposed to hold in all maladies, yet in reality it is but in very few, that the blood (and I might add, the urine) affords to the practitioner much useful information."

With every respect to the memory of this learned physician, I must consider that the unqualified sentiments here expressed, are both devoid of truth and of hurtful tendency, as leading us away from practical observation, the most ge-

nuine and useful source of knowledge. We should certainly avoid running into the speculations and delusions of the humoral pathology, in the doctrines of which so much more has been assumed than proved; but yet I am persuaded, that a study of the nature and appearance of the blood drawn in diseases, will teach us important information of the real state of the power and the action of the heart and arteries, and of the condition of the system itself. Is not every individual solid in the body derived, supported, and continued by means of the blood? But to return to the consideration of the buffy coat.

It is undoubtedly true, as a general circumstance, that in the same proportion that the buffy coat of the blood appears to abound, the clot being remarkably cupped, as it is expressively called, with edges inverted, so does the inflammatory diathesis prevail; and indeed, short of such strongly marked evidence, in those cases in which the signs of internal inflammation are obscure, the appearance of the blood as exhibiting the buffy coat, or being wholly free from it; or in

its texture as being remarkably firm, or the contrary, very materially serves to assist our diagnosis. In forming such diagnostic opinion, I must again desire, that reference be made to all the circumstances which have an influence on the particular formation of the coagulum.

An increased rate of the circulation, merely, does not occasion a larger proportion of the fibrin to be found in the blood, as is proved by Experiments lxxxiii and lxxxiv. On the contrary indeed, in this example, the largest proportion of fibrin was found in the blood drawn before the exercise. The increased proportion of serum, together with its higher specific gravity, concurring with the diminished proportion of fibrin, is a fact worthy of observation.

It requires the continuance of disease to give rise to the buffy coat. It will not be found in blood drawn in the first few hours of inflammatory action. I have just now mentioned the negative effect of exercise; but also, it seldom happens that the blood is sizy in simple continued fever, or certainly not in the beginning of the

fever. We may expect to find this condition of the blood in those diseases which are attended with muscular wasting ; for example, in diabetes, and above all, in phthisis pulmonalis. Dr. Watt, in his work on Diabetes, mentions the remarkable prevalence of the buffy coat in this disorder ; and, probably, was on this account encouraged in his views of treating diabetes by frequent bleeding. In the early period of pregnancy, the blood usually presents some degree of buffy coat ; and, indeed, it has frequently been regarded as a diagnostic of pregnancy. I attribute its appearance to the degree of muscular wasting which takes place under these circumstances ; agreeably to my former explanation, of the fibrin being retained in excess in the circulating blood, instead of being distributed in the usual proportion to the muscular fibre.

It deserves further discussion, how far the appearance of the buffy coat is to be taken as a fixed practical rule for the use of the lancet? It appears to me, that while on the one hand it is to be received as strong evidence of the inflam-

matory diathesis, we must, on the other hand, be careful not to be too much influenced in the repetition of the venesection by such appearance of the blood. In many diseases it will continue to predominate under circumstances of such great debility, that it would be highly injurious to pursue the practice of bleeding. Even to the dying hour of the consumptive patient, the blood does not fail to exhibit the buffy coat, more or less considerably. In the treatment of severe pleurisy, its continued appearance does not alone warrant incessant bleedings. We must always make a comparative estimate between the action of the vessels arising from disease, and power of the constitution. Whatever may be the medical treatment, a little time is required to allow a change of diathesis to take place ; and, although, during the state of active symptoms, the free use of the lancet is the sine quâ non of practice, we are not to consider that the taking away blood constitutes the only means by which we can alter its condition ; or, more properly, by which we can remedy the disorder of the system. Medi-

cine, suitable diet, repose, and a little time, will accomplish much. In prescribing venesection according to real symptoms and circumstances, we shall most probably avoid error; and it is incumbent on us to take a careful guidance from the mere continued appearance of the buffy coat. I repeat, that it will frequently be found connected with such serious debility of the constitution, that to continue to rob the vessels of blood on account of its being sizy, would be dangerous to the life of the patient.

It is obvious that we are led to the further use of the lancet when the buffy coat of the blood appears; because it is so commonly associated with inflammatory action; but, otherwise, according to the explanation which I have attempted, that the excess of fibrin in the blood is simply owing to a suspension of its ordinary distribution to the fibrous textures, we should not, in a theoretical view of the question, consider depletion from the vessels as indispensable. A very free and continued employment of bleeding in acute rheumatism, is seldom a successful practice, although

it is notorious that every cup of blood would present the appearance of the buffy coat.

In regard to the different proportions of fibrin in different cups of blood, filled at the same bleeding, I wish to make a few further remarks.

Mr. Hewson was of opinion, that the increased action of the blood vessels in inflammation has the effect of thinning the lymph; to lessen its disposition to coagulate; and in that way to cause the appearance of the buffy coat; for the reason, that the red particles, being the heaviest part of the mass, subside and separate from the thinned coagulable lymph, which we now call fibrin. His words are (page 129, vol. i.), "for when the vessels act more strongly than they do in health, the lymph is proportionably more thinned, and is less disposed to concrete; and when the vessels act more weakly than in health, then the lymph is proportionably thickened, and is more ready to concrete."

The ingenious Author was not aware of the real fact of the extraordinary difference in the

quantity of the fibrin in the different cups of blood. He adverts only to its density and tenuity.

The explanation of the fact, which I have suggested at page 119, appears to me deserving of consideration, if not entitled to be received with confidence.

The experiments which I have related, sufficiently serve to show that the proportion of *carbonic acid gas* not only varies remarkably in the blood of different individuals, but also varies in the blood of the same individual at different parts of the twenty-four hours. In the experiment described at page 107, the result shows a double quantity of gas in the blood drawn after the dinner meal.

I have frequently heard patients complain of a peculiar sensation of fulness of the veins, as if they were distended with air. When the case is urgent, not only the veins, but the skin also gives the sensation of tightness and distension. It seems to me highly probable, that, in these circumstances, there is an excess of carbonic acid in the

blood; and if this opinion be correct, it would surely be useful to prohibit the use of all liquors which contain much of fixed air. We may extend this prohibition to persons suffering from fulness of habit, or any condition of the vessels in which it is of importance to counteract congestion of blood, or distension of the blood vessels in any part of the circulating system.

In the Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, a case is related by Dr. Baillie (whose loss we so recently mourn), of Emphysema, not proceeding from local injury. After stating that the emphysema did not arise from any accident, as that of a broken rib wounding the lungs, nor from putrefaction, the Author conceives the probability that the small blood vessels distributed on the cells of the cellular membrane, may have some power, under particular circumstances, of secreting air. He supposes "the air to be generated in the blood vessels themselves, and to be thrown out in the form of a secreted fluid into the cellular membrane." And again, "as being

formed from the blood itself by some peculiar arrangement of its parts, and conducted into the cells of the cellular membrane by very small vessels."

At the period when this paper was written, the fact of carbonic acid gas being one of the constituent parts of the blood, most probably was not known.

In referring to the effect produced on the blood by different *chemical agents*, the reader may naturally entertain the question, how far we may reason chemically upon the influence of such substances, administered internally? Seeing that the alkalies and common salt, freely applied, entirely prevent the coagulation of the blood; and that the mineral acids promote coagulation; we must necessarily conclude that the same agents will act in a very opposite manner as medicines. I have not had the opportunity of examining the condition of the blood in scurvy; but the fact is well known, how much that disease is a consequence of the sole use of salt provisions, and how much the cure is found in the

free employment of acids. That I may not be misunderstood in my medical reasoning, I wish distinctly to observe, that in the treatment of disordered states of the circulating system, I consider, that our means are chiefly to be directed to the object of effecting a change in the economy of the vessels, and of the vital functions; and that the alteration which we may be able to produce in the blood itself will be for the most part secondary, and as a consequence of the amendment of the functions. But notwithstanding this pathological view of the subject, I contend that we ought, both theoretically and practically, to have some regard to the tenets of the humoral pathology; and allow ourselves to reason upon the probable chemical influence of different medicinal agents on the blood itself.

I have now to mention, with particular satisfaction, the remarkably efficacious properties of a saturated solution of the sulphate of alumina (alum) as a styptic to bleeding vessels.

With the obliging assistance of Mr. Wood, Lecturer on Chemistry at the Middlesex Hos-

pital, I put its virtues strongly to the test, in the following experiment upon a dog. We exposed by dissection, arteries of different capacity, and punctured them. In comparative trials, we used saturated solutions of acetate of lead, sulphate of copper, sulphate of zinc, and of pure crystallized nitrate of silver; tincture of galls, nitric acid diluted, and Ruspini's styptic*. The sulphate of copper, and still more Ruspini's styptic, had an effect upon the hemorrhage proceeding from very small arterial branches, but not the slightest upon an artery of considerable size, which bled vehemently. On pouring the alum solution on this vessel, we were equally surprised and gratified to witness, that in about a quarter of a minute, the bleeding entirely ceased; and that it was thus effectually restrained by means of the tenacious coagulum so promptly formed, operating as a plug to the mouth of the vessel, was proved several times, by sponging away the coagulum,

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* I have reason to believe that the active ingredients in this styptic, are galls and sulphuric acid.

seeing the violent renewal of the hemorrhage; repeating the application with perfect success; and so on.

I have had some convincing opportunities of the benefit of this styptic solution, applied to the human subject. I shall relate a few instances.

A crucial incision was made upon a tumour, partaking of the nature of a carbuncle, by which two arterial branches were divided, of such considerable size, that, in a few minutes, a pint of blood was lost by the hemorrhage. The ordinary means had failed, when the saturated solution of alum, applied by means of a thick compress of lint, and with the aid of less pressure than had been used in the previous treatment, proved immediately and perfectly successful. The application caused some smarting, but not pain.

A person was cupped at the temple, and, by accident, a branch of the temporal artery was partially divided, so as to cause considerable hemorrhage, which had nearly ceased, from the application of lint, adhesive plaister, and strong pressure. Soon, however, the bleeding returned, and va-

rious styptics were applied with such little success, that the surgeon divided the artery, and thus arrested the hemorrhage. However, in an hour after, the bleeding commenced again, profusely. The usual styptics and pressure were repeated without success. In this dilemma, the saturated solution of alum was used (in consequence of a communication which had been made by Mr. Wood, who had assisted in my experiment), and it proved immediately and perfectly effectual.

A patient suffering severely from inflamed hæmorrhoids, which were very protruding, applied some leeches, and promoted the bleeding in the usual manner by sponging with hot water. He retired to rest, in the belief that all troublesome discharge of blood had ceased; but he soon discovered that an active hemorrhage was going on; and, upon visiting this patient, I found that arterial blood was flowing very fast from one of the leech bites. A thick compress of lint wetted in the before-mentioned solution, with moderate pressure, succeeded instantly, and

no further inconvenience was experienced. The solution should be used rather warm, as being still more coagulating than when cold, agreeably to the experiments detailed in the beginning of this Essay. In the case of a difficult hemorrhage, cold may be applied with advantage to the neighbourhood of the bleeding vessel, in order to diminish vascular action. Such practice would be perfectly consistent with the application of the warm solution to the bleeding vessel itself.

In some cases of hemorrhage from the unimpregnated uterus, and especially of the passive kind, I apprehend that the free injection of this solution would be attended with the most important advantage; and indeed I am sanguine enough to think, that, under certain circumstances of hemorrhage after delivery, and the removal of the placenta, it will promptly prove a remedy for a state of imminent danger, when other means would probably fail. I should recommend the injection to be used tepid, by means of a proper and efficient apparatus; while, at the same time,

if the urgency of the case should require such an additional step, cold might be employed externally on the abdomen, in the usual manner.

In conversation with my friend Mr. Clarke, of Saville Row, I received the high sanction of his opinion, that the treatment, suitably applied, would be very proper. He does not apprehend that in the case of uterine hemorrhage, any risk of mischief would arise from the free injection of this solution.

I conceive, therefore, that every practitioner in midwifery should constantly provide himself with a quantity of the alum salt; and then, with the mere addition of warm water, he is promptly furnished with a powerful remedy, under circumstances which are always anxious, frequently very alarming, and sometimes fatal.

In failure of having a suitable injecting apparatus, a common pipe and bladder would not be very inappropriate for the purpose of injecting the solution.

I am well aware that, for centuries past, alum

has been used as one of the ingredients in styptic compositions; but according to all the information which I can collect, it has never been employed in the manner that I recommend it, as a saturated* solution. The whole merit of the remedy consists in its being used in this degree of strength, and in the fluid form. As I have already pointed out, the solution when used tepid more quickly produces a coagulum, than if employed cold.

I have met with some opportunities of giving this salt internally. In a case of hæmoptysis, and in a case of hæmatemesis, it succeeded perfectly. In each instance, the hemorrhage ceased entirely after the administration of a few doses of the medicine. In the case of bleeding from the lungs, the symptoms were slight; but the vomiting of blood, and its discharge by the bowels in the other case, were very urgent. I am here

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* I find by experiment that one ounce of cold distilled water holds completely in solution, 31 grains of alum; and infusion of roses, 34 grains.

reciting a fact, but do not wish to be considered as recommending the use of an astringent styptic in every case of this description. The infusion of roses with mucilage of gum acacia and a little syrup, was the vehicle which I prescribed; and the alum was taken in free doses, without any inconvenience to the stomach.

I shall feel much obliged to my professional brethren, to favour me with any communication respecting the effects of the treatment in question, whether successful or otherwise. The truths of practical medicine become established by the collected testimony of the many; and the partial recommendation of remedies, which springs from individual authority, must always be received rather as a proposition, than as conclusive evidence.

FINIS.

Joseph Mallett, Printer, 59, Wardour Street, Soho, London.

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